

What Will Happen in Stockholm?

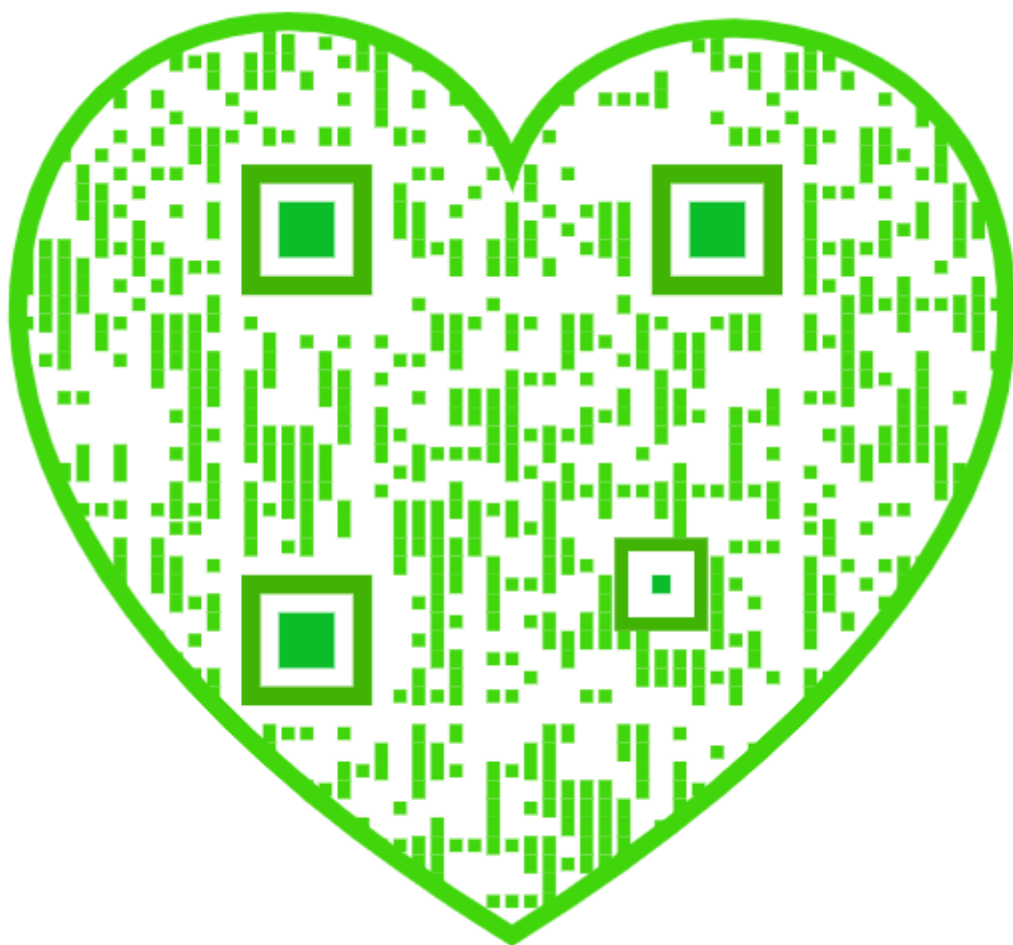
THE United Nations Conference on the Human Environment, which opens at Stockholm next week, is all things to all men. To some people, it is a welcome sign that the United Nations has now taken a grip on some of the environmental problems which have perplexed the public, some governments and some professional people for the past few years. To others, it is a potential danger, perhaps because it may give governments a practical demonstration that in dealing with the environment, lip service goes as far (and may be much cheaper) than practical action. There is also a danger that the conference will serve to give governments of all kinds a distorted sense of priorities, for even if the jumble of environmental problems now widely talked about may contain matters which merit urgent attention, very little has so far been done by public discussion to persuade governments away from their flair for tackling first of all the problems which are most easily soluble (but which may not be by any means the most important). These, however, are merely the cross-currents of anxiety about Stockholm which are to be found in countries prosperous enough to be able to afford to put anxiety about the environment near the top of their conscience. Elsewhere the complaints that the conference is yet another device by means of which the developed countries seek to keep the less developed in thrall have been only partially subdued by the patient work of the planning and drafting committees in New York, while it remains anomalous, to say the least of it, that population growth does not figure on the agenda for Stockholm. The truth is, however, that the Stockholm conference could have an important and beneficial influence if it can create a climate in which national governments—the sole members of the conference—are somehow persuaded to tackle more urgently a number of practical tasks on which they have dragged their feet for several years. But the influence of the conference will be calamitous if it becomes a platform for open political squabbles or if it becomes a competition in platitudinous overstatement.

The Preparatory Committee has not done much to help avoid the second danger. The conference documents so far available beg too many questions for comfort, and are at the same time suffused with too much drama. (Why, in any case, must human beings consistently be referred to in the singular, as Man?) The first clause of the declaration on the Human Environment which the conference is invited to adopt is, for example, curiously tautologous. It says that "Man has the fundamental right to adequate conditions of life, and an environment of a quality which permits a life of dignity and well-being, and bears a solemn responsibility to protect and enhance the environment for future generations". The trouble, of course, is that what is now described as a "fundamental right" has for most of human history been regarded as a goal to which well intentioned people should aspire. And does not the use of "environment" in this sense include not merely the natural environment but the built environment as well as the stream of industrial products from frequently polluting factories? And if that is so, does it

not follow that the process of protecting and enhancing the environment for future generations must include not merely policies for the conservation of living things and natural resources but the formation of capital and the strengthening of the stream of scientific discovery which is, among other things, the foundation of much of what the environmental movement at present abominates? In short, are the drafters saying more than that they, like all liberals, consider that modern society should be more just and prescient? The trouble, of course, is that any government could put its name to such a declaration without fearing that its freedom of action, however illiberal and short-sighted, is compromised.

Like many other recent pronouncements on environmental subjects, the Declaration on the Human Environment is also curiously innocent of the complicated economic balances which must be struck between apparently desirable but frequently conflicting policies for the improvement of the environment in the widest sense. The document says, for example, that "the non-renewable resources of the Earth must be employed in such a way as to guard against the danger of their future exhaustion". What does that imply? With petroleum, for example, where it is more than probable that natural reserves will be relatively much more scarce than now in about a century, are the members of the Stockholm conference being invited promptly to forego the use of petroleum products of all kinds? Should the chief oil-consuming nations impose a self-denying tax on their consumption, or trust that the OPEC countries will succeed in putting up the price sufficiently to restrict the pace at which petroleum is at present mined? Or might it be sufficient for the developed nations to push ahead as quickly as they can with the programmes of research by means of which petroleum products might be extracted from oil shales coal, or even to be more energetic in their pursuit of thermonuclear research? These questions are neatly buried by the form of words on which the drafters have settled, but they are none the less important for that reason. Unhappily, there is very little sign so far that the Stockholm conference will be equipped to deal with them seriously. And what will it have to say about the earnest plea that "Man and his environment must be spared the serious effects of further testing or use in hostilities of weapons, particularly those of mass destruction"? Nobody will quarrel with the sentiments, but are the delegates to Stockholm prepared seriously to discuss what has become known in disarmament circles as General and Complete Disarmament (GCD for short)?

On the face of things, at least, the planners of the Stockholm conference have spread their net too wide. Even if they have had the good sense to recognize (in the declaration) that economic development is not merely necessary but desirable (which will lose them many friends), there would have been great benefits in a more modest statement of the nature of the environmental problem, as well as in a more tangible statement of the useful steps which can now be taken. The difference between the conference



and its critics stems from the view which is taken not of the seriousness but of the urgency of the environmental problems which have in recent years become preoccupying. Much depends on the assessment which is made of the time-scale on which environmental disaster of one kind or another would afflict the world. If it can be held that there will be environmental catastrophe of some kind or another in the next few decades "if present trends continue", then it is clearly important that people should quickly abandon other preoccupations for the sake of fending off disaster. But if the time-scale of potential disaster is much longer, it is plainly possible to work towards an extension of present arrangements for the proper management of the environment (which at present includes such things as the regulations which exist for preventing the spread of infectious diseases, not to mention controls of the exploitation of international fisheries) while at the same time continuing with other urgent matters, among which the economic growth of developing nations must take pride of place. To be fair, the proposals for steps which might be taken to be presented at the conference in Stockholm are modest enough—the trouble is that they are clothed in rhetoric borrowed from those who think the world is on the point of collapse.

What steps might be taken? The "action plan" now being circulated concentrates on three fields in which governments might collaborate. First, there is a set of constructive proposals for monitoring the condition of the environment. Then there are schemes for the coordinated management of the environment—not merely city planning within nations but the proper management of international fisheries. Finally, there are educational proposals that, however valuable in themselves, suggest that the planners have not counted the great number of professionals with a finger in this pie already. Yet something may come of these proposals.

Doldrums at Santiago

THE UNCTAD conference at Santiago has turned out to be as great a disappointment as the sceptics were fearing six weeks ago (see *Nature*, 236, 361; 1972). As at the two previous meetings of the United Nations Conference on Trade and Development, the developing countries have failed to agree on a common policy, with the result that they have been unable to face the richer nations of the world with a tangible bill of goods about which serious negotiations might have taken place. Instead, the conference appears to have finished in a blaze of fierce confrontation between the haves and have-nots, an occasion which served only to dramatise the extent to which the expressions of good intentions by the developed nations of the world are falsified by their parsimony. The meeting has been such a shabby affair that the survival of UNCTAD must now be seriously in doubt. Nobody will dispute the potential advantages of a forum for collective bargaining between the developing countries and the developed on questions such as tariff preferences and the means of financing international trade with and among the developing countries. It has always been on the cards, however, that either the developing countries would fail to find a common negotiating position or, if they did, that the reactions of the developed countries would be intransigent. At Santiago, both weaknesses have been painfully

apparent. It is especially reprehensible that the developed countries resisted, as they appear to have done, the argument that the allocation of Special Drawing Rights on the International Monetary Fund to developed countries should be somehow linked with the scale of their development assistance—that proposal is now remitted to the IMF for further and no doubt inconclusive study.

The importance of trade in the development of the poorer countries is now well understood. The developing countries are distinguished from the rest of the world not merely by their rapid rate of population growth but by the comparatively small proportion of their resources invested each year in capital projects. In advanced communities, the proportion of the national product reinvested as capital is usually more than 12 per cent and may frequently exceed 15 per cent; in the developing countries, the rate of capital investment rarely exceeds 5 or 6 per cent. The reasons for this discrepancy are not hard to find. When the urgent tasks are to provide food and other bare necessities for a rapidly growing population, it is no wonder that a developing nation can spare only a small part of its resources for projects that will bring more distant benefits. Except in agriculture, where the benefits of investment can be rapid, the experience of developing countries in the past few decades suggests that the rate of investment works out at roughly three times the rate of economic growth it generates—in other words, by investing nine per cent of the national product in sensible capital projects, a developing country can reasonably hope that its economy will grow at three per cent a year—just a little faster than the rate of population growth in many countries. But since the rate of domestic saving is not usually enough to sustain such a rate of capital formation, even such a modest rate of economic growth must be sustained by capital from abroad.

This is the principle on which most foreign-aid programmes are based. Organizations such as the World Bank, for example, are willing to finance schemes for capital investment but set their faces against financing immediate consumption and that, with some reservations, is how they should conduct themselves. The funds which developing countries earn by means of exports are, however, in this connexion especially valuable. For one thing, to the extent that they represent a diversion of resources from consumption, they augment the total of domestic saving. But foreign exchange is also necessary if developing countries are to buy abroad the capital goods needed for investment projects of all kinds. In short, if the developing countries are unwilling to increase dramatically the scale on which they provide capital for developing countries, then they must help them somehow to trade abroad.

The post-war history of attempts to liberalize trade with developing countries is thoroughly depressing. The percentage of world trade consisting of exports from the developing countries has decreased from 27 per cent in 1953 to 19 per cent in 1967. During the same interval, when world trade as a whole has multiplied three-fold, exports from developing countries have not quite doubled, from \$21,000 million a year to \$40,000 million a year. The developed countries are responsible for all kinds of restrictions on this potentially beneficent growth. Throughout the developed world, there are notorious restrictions on imports of textiles from developing countries, for example. In many countries, raw materials can be imported free from tariff, but manufactured products

made from the same materials are subject to tariffs which—although they may be arithmetically quite small—are a powerful discouragement to the development of rudimentary manufacturing industries in developing countries (and, for that matter, an important extra economic burden for the developed countries at the same time).

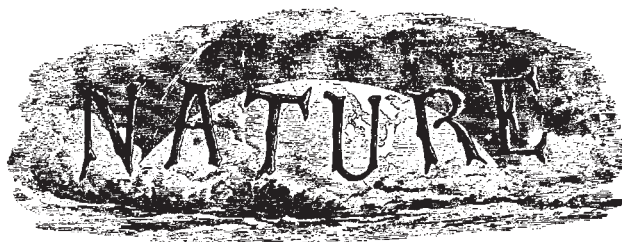
Over the years, there have been several attempts to remove some of these obstacles to international trade with the developing countries, but there are few successes to report. The root of the trouble is that the developed countries are prepared to consider seriously only those measures for the liberalization of trade which do not conflict with their own policies for the protection of traditional industries, among which agriculture and textiles are conspicuous. And recent experience in the United States shows how quickly prosperous governments will jump for tariffs and quota restrictions if, for example, it seems that developing countries are about to make a commercial success with exports of, say, boots and shoes. Although there has been some progress with commodity agreements which help to stabilize the prices of some of the commodities exported by some of the developing countries, very little has been done to give exporters from the developing countries more ready access to the richest markets in the world. And the prospects for change are gloomy—the fierce protection of European agriculture within the European Communities is a sufficient proof of that. Plainly there is very little hope that the years ahead will see more than mere argument about the proposal that imports from developed countries should elsewhere be free from tariffs of all kinds, although it would be less sickening if the developed nations were not so consistently to base their rejection of this proposal on the argument that it would contravene the regulations of the General Agreement on Tariffs and Trade (GATT)—a set of rules which they are all too ready to break in their own interests.

What, in the circumstances, is to be done? Although the Santiago conference asked that the question of Special Drawing Rights should be considered by the next meeting of the International Monetary Fund in the autumn, there is very little hope that the intransigence of the rich countries will have evaporated by then. The best chance is that there may now be some progress on the removal of impediments to trade with the developing nations within the framework of GATT, which does at least provide machinery for the orderly reduction of tariffs and the removal of quota restrictions. In the mid-1960s, there was reason to hope that discussions within GATT would make possible a substantial increase of exports from developing countries, but in the end the horse-trading (as it has to be) of what was called the Kennedy Round of tariff reductions was chiefly of benefit to exporters in the advanced communities. Is it possible, now, that the advanced communities may be shamed by the disgraceful goings-on at Santiago to use GATT as a means of offering substantial tariff reductions which may help to redress the present unseemly balance between the rich and poor? That is the best hope, and the European Communities could play an especially important part in this development, if only because the present president of the Commission, Dr Sicco Mansholt, has set himself up as a liberalizing force. On paper at least, the British Government should be an influence in the same direction—Mr Michael Noble, the Minister of Trade, uttered cheerful platitudes on the subject at Santiago. But what is needed now is an initiative by the developed countries to agree on a set of

measures for liberalizing trade with the developing countries, the discovery of some device for forcing the United States to toe a liberal line and a determination not to postpone implementation until the developing countries have agreed precisely what they want.

There is also a case for sober and urgent reconsideration of the scale on which capital resources are at present made available to developing countries. Previous UNCTAD conferences have agreed that roughly one per cent of the Gross National Product of the developed countries should be devoted to development assistance, and that 70 per cent of this should come from public sources, not private companies. Scandalously, the performance of most developed countries still falls short of these modest targets. In 1968, for example, the net cost of foreign aid to the United States amounted to 0.37 per cent of the GNP, the United Kingdom's performance was a little worse and the average net foreign aid from public sources for the thirteen members of the Development Aid Committee of the OECD worked out at only 0.40 per cent of the GNP. Yet there is every reason why the targets which the developing countries have so far failed to reach are at once far below the capacity of the developing countries to make use of capital and far below the scale on which an improvement in the economic position of the developing countries could be brought about in a measurable period. If one per cent, in total, is the present target, what is wrong with 2 per cent? And why should not the developed countries be compelled not merely by shame but by enlightened self-interest to channel increasing proportions of their foreign aid through the multinational institutions now operating in this field? Unless they do so quickly, it is hard to see how the disasters of which they all speak can be avoided.

100 Years Ago



ELECTRIC VALENTINE

TELEGRAPH CLERK ♂ TO TELEGRAPH CLERK ♀

"THE tendrils of my soul are twined
With thine, though many a mile apart;
And thine in close-coiled circuits wind
Around the magnet of my heart.

"Constant as Daniell, strong as Grove;
Seething through all its depths, like Smee;
My heart pours forth its tide of love,
And all its circuits close in thee.

"O tell me, when along the line
From my full heart the message flows,
What currents are induced in thine?
One click from thee will end my woes."

Through many an Ohm the Weber flew,
And clicked this answer back to me—
"I am thy Farad, staunch and true,
Charged to a Volt with love for thee."

Published anonymously by Clerk Maxwell.

From *Nature*, 6, 83, May 30, 1872.

OLD WORLD

What the Public Thinks

THE British preparations for the Stockholm Conference on the Human Environment are nearing completion. The past ten days has seen a flurry of activity, with the publication of the three outstanding reports of the working parties set up by the Secretary of State for the Environment a year ago to sound out public opinion on different aspects of the environment. (*Polution: Nuisance or Nemesis*, chairman Sir Eric Ashby; *Human Habitat: How Do You Want to Live?* chairman the Countess of Dartmouth; *Fifty Million Volunteers*, chairman Mr Denis Stevenson all published by HMSO.)

Nuisance or Nemesis, the first of the reports to appear, contains no detailed recommendations. According to Sir Eric Ashby, the document is "not a report but a piece of reporting" which reflects public opinion, and so "is a summary of a spectrum of views". As such it contains some strong statements with little apparent justification. In particular its excursion into the world of nuclear power is destined to create mild concern among the planners of Britain's power programme who are planning that 60 per cent of all power should be obtained from nuclear sources by the year 2000. The report says that "it would be prudent to slow down the nuclear power programme until we have solved the waste disposal problem or until we have developed fusion power or some other type of reactor which does not leave dangerous wastes behind it".

The working party justifies its attitude by saying that man may put all his eggs in the nuclear basket before he discovers that a solution cannot be found to the radiation hazard. Then, Sir Eric and his colleagues continue, there would be political pressures to ignore these hazards.

The working party feels that a voluntary curtailment of the population is needed so that the population at some future date will not "be limited by shortage of food or resources". The working party was divided on whether a drastic revision of society's values is needed, with some members thinking "a fundamental and painful restructuring of our industrial society is necessary".

The report states that "pollution control involves many difficult and debatable decisions and governments... are unlikely always to judge correctly". The working party continues that the decisions and the underlying facts must be open to scrutiny by scientists, the press and the public to avoid costly mistakes.

The report ends in a flourish with the working party expressing the hope that "polution [will] be brought under control and mankind's population and consumption of resources be steered towards a permanent position of sustainable equilibrium". The final words are a warning of impending doom if this advice is not heeded; "unless this is done, sooner or later—and some believe that there is little time left—the downfall of civilization will not be a matter of science fiction. It will be the experience of our children or our grandchildren".

In contrast to Sir Eric Ashby's report, Lady Dartmouth's working party makes more than fifty recommendations on which the members are in complete agreement. They are realistic enough, however, to say that they hope the government will "accept and implement some of our recommendations". Mr Eldon Griffiths, Minister of State at the Department of the Environment, said

that the government accepted some of the recommendations, wanted to think about some others and wanted to argue about the rest.

Lady Dartmouth said that what had surprised her in her travels about Britain were the differing attitudes of local authorities. The contrast is so great, according to Lady Dartmouth, that the good authorities are very good but the bad authorities are very bad indeed. In some places she said that she was "shocked to see what had been done to the habitat in the name of progress".

The working party recommends that people be encouraged to return to live in the middle of the towns. Lady Dartmouth does not favour "eroding the green belt" and she pointed out that there is plenty of land within cities for houses to be built. In the past, places "have been wrecked by lack of thought with planning permission injudiciously given".

EUROPE

Fears for the Universities

from a Correspondent

LORD BOWDEN, principal of the University of Manchester Institute of Science and Technology, last week welcomed the attempts being made to define common standards for many European professions. But he also issued a note of warning about the way the negotiations have been and are proceeding.

A group of civil servants has been at work since 1962 in Brussels attempting to set standards and the group has produced forty draft directives which define how men should be educated if they want to be accepted within the community. (The group says, for example, that a man may not be a dentist unless he has taken a course which lasts for 5,600 hours spread over seven years and has allocated almost every hour to a particular topic.)

These directives, according to Lord Bowden, have been produced without regard for the national interests of members of the community and there are indications that the work is now being done with indecent haste in order to finish before Britain joins the community next January.

Lord Bowden also contrasted the relative tranquillity of the British university scene with the more troubled campuses elsewhere, particularly Ger-

many and the Americas. He said that even five years ago, senates were all-powerful and totally dominated by professors, but that they have now been swept away. All German universities, he went on, are governed by completely new councils, some of which include representatives of the local community, non-academic staff, technicians, cleaners and porters.

After tracing the reasons for overcrowding in European universities, where the student-staff ratio has, in some faculties, deteriorated from fifteen to one to fifty or even a hundred to one, Lord Bowden expressed some alarm at the prospect of Britain joining the European Economic Community. Students and professional men are able to move freely throughout the whole community, so Lord Bowden wondered whether British universities would be inundated by people who have taken school leaving examinations elsewhere in Europe and believe themselves to be entitled to university places in Britain.

Lord Bowden concluded by saying, in no uncertain terms, that Britain should reject the whole European university scheme out of hand. British universities, he said, cannot and should not be prepared to accept a system which totally inhibits academic experiments, which puts a straitjacket on both universities and professions and which is totally out of keeping with all Britain's most treasured academic traditions.

NUCLEAR POWER

Any Day Now?

A PLEA for the reorganization of Britain's nuclear power industry was put to Mr Tom Boardman, Minister for Industry, last week by the Institution of Professional Civil Servants (IPCS).

The industry has been in a state of suspended animation for more than eighteen months while Mr Peter Vinter, a deputy secretary at the Department of Trade and Industry, has conducted a review of thermal reactors to help ministers decide what type of reactor Britain should plump for to fill the gap in the nuclear power programme between the second generation advanced gas cooled reactors and the breeder reactors which should start generating commercial quantities of power in the 1980s.

IPCS—which represents the majority of scientists and technicians in the nuclear power industry—told Mr Boardman that it is vital that a decision to go ahead with a British thermal reactor is taken without delay. The institution also told the minister that it would be fatal for the industry in the long run if Britain relied on American thermal reactors to fill the gap. The choice lies between three British reactor types, none of which is yet commercially proven and two of which—the steam generating heavy water reactor and the high temperature reactor—are not yet fully developed (the third type being the AGR), and two American light water reactors, both of which are well tried although neither is technically very advanced and neither of course has been built by the British consortia.

The institution also told Mr Boardman that the two nuclear design and construction consortia—British Nuclear Design and Construction and the Nuclear Power Group—should be merged but emphasized that British Nuclear Fuels Ltd, the state-owned fuels company, must be part of the new group if staff is to have confidence in the new consortia.

The IPCS also insists that the staff of the nuclear industry has as much right to be consulted about the impending reorganization of the industry as any other body and said that it would oppose any restructuring “worked out behind closed doors”.

The delegation reported after the meeting that its representations were well received and that Mr Boardman had promised to consult them “at the appropriate time”. He also informed them that the government fully supports the continuation of Britain's nuclear power industry.

Although the Vinter report has been in ministers' hands since early March,

no decisions have yet been made public. It is apparent that Mr Vinter's brief was far wider than was thought at first and it seems likely that the decision has been complicated by the need for Britain to think out the whole of its future energy policy, particularly with regard to the part coal is to play. A slight fall in the predicted electricity demand for 1977 to 1978 has allowed the government a certain amount of breathing space in which to make its decision, which has been promised “as soon as possible”.

ESRO

Base for Britain

THE four-year-old National Centre of Tribology at the United Kingdom Atomic Energy Authority's research establishment at Risley is to become ESRO's new European Space Tribology Laboratory (ESTL). The National Centre has won the £397,000 contract to build and operate the laboratory in the face of stiff competition from twelve other European organizations. When the programme is in full swing the contract, which will run for four years, will make up between 30 and 40 per cent of the centre's work.

To date the National Centre of Tribology, child of the now defunct Ministry of Technology, has worked with a combination of Atomic Energy Authority money and industrial contracts on the science of friction and lubrication. Its success has led to the award of the ESRO contract, the chief purpose of which is to establish the

long term reliability of rubbing surfaces in satellites.

To fulfil the contract, the laboratory will be equipped with six vacuum test chambers to test six slow speed solar paddle drive mechanisms for applications satellites for a seven year period. Eight smaller high vacuum chambers will be installed for the support programme, which is to include evaluation of solid and liquid lubricants for different types of space mechanisms. Work on de-spin mechanisms and momentum wheel bearing assemblies will also be undertaken. Assemblies tested will be full-scale prototypes of real satellites.

The test facilities will be particularly relevant to the development of the telecommunications satellite planned by ESRO. The first assemblies to be tested are all British products, but it is hoped that European products will be tested later and that European aerospace companies will place orders with the laboratory.

Industrial contracts already make up half the National Centre's work, and Dr Harold Hardy, Director of Reactor Fuels and Materials, UKAEA, said last week that the centre's experience of working to tight timetables and close financial control should stand the new European laboratory in good stead. The centre has won over 200 contracts worth almost £250,000 in its four years, and almost half its customers have come back a second time.

The contract has been awarded for an initial period of four years because ESRO's financial future after 1975 is uncertain.

COMMON MARKET

Aims for Energy

THE Commission of the European Communities has come out with the suggestion that the six member countries should aim to have 100,000 MW of nuclear generating capacity installed by 1985. The commission says that the quantity of electricity generated by nuclear means in the EEC (5,000 MW at present) should be increased to at least 12,000 MW by 1975 and to at least 45,000 MW by 1980. These targets are much lower than those set by the commission as recently as 1967 when it envisaged the installation of some 60,000 MW by 1980.

At 1970 prices, this new programme would cost about \$1,650 million a year in 1975, and more than \$2,200 million a year five years after that. The amount earmarked for new nuclear plants in 1972 is, by comparison, about \$1,000 million.

The capacity of electrical plant in use in some European countries at the end

of 1969 is shown in the Table. In the EEC, electricity accounts for about 6 per cent of the total energy consumed, compared with crude oil and its products (60 per cent), natural gas (11 per cent) and coal (23 per cent). Although

Capacity of Power Plant in Europe (1969)

	Conventional GW	Nuclear GW	Hydroelectric GW
Germany	40.0	0.8	4.6
France	17.8	1.6	14.5
Italy*	14.2	0.6	13.2
Netherlands	9.6	0.05	0
Belgium	6.2	0.01	0.06
Luxembourg	0.2	0	1.0
EEC total	88.0	3.1	33.4
Britain	51.3	4.0	2.2

* Italy also generates 0.4 GW by geothermal means.

the consumption of natural gas increased by about 27 per cent in 1971 (compared with 1970), the quantity of electricity used in the EEC fell by 2.5 per cent and the amount of coal consumed was down by nearly 7 per cent.

NEW WORLD

AEC Hedges its Bet on Waste Disposal

by our Washington Correspondent

THE US Atomic Energy Commission has a growing problem on its hands—how to dispose of highly radioactive nuclear waste material from power plants and to keep it isolated from the environment for several thousands of years. Until last year, the commission believed it had the problem solved with a proposal to bury the wastes in a disused salt mine in Lyons, Kansas, but technical difficulties have forced it to abandon that site and search for a more suitable one. The commission was therefore likely to be faced with the embarrassing possibility of receiving its first batches of hot solidified wastes from the nuclear power industry at the end of the 1970s, but having nowhere to put them. That is why the AEC announced last week that it is proposing to build surface bunkers to store hot wastes in the early 1980s. The proposal represents some backtracking by the commission for it had previously opposed long-term storage of such waste above ground because of the need for constant surveillance to guard against leakage.

The commission has, however, not completely turned its back on the suggestion of storing solidified wastes in disused salt mines, and is ready to construct a pilot plant to test the concept as soon as a suitable disused mine can be found. Hedging its bets still further, the AEC has also signed a contract with Battelle Northwest Laboratories to evaluate some of the other weird and wonderful suggestions for disposing of nuclear wastes that have been aired by various authorities, including the chairman of the AEC himself.

The wastes are derived from the fuel reprocessing plants which reprocess and refine spent fuel elements from nuclear power plants. Their chief danger lies in the fact that not only are they highly radioactive—the solutions in which they are at present stored can have up to 10,000 Curies per gallon—but that they contain long-lived radioisotopes, particularly strontium-90 and caesium-137, which have half lives of the order of hundreds of years, and plutonium-239 which has a half life of 250,000 years. It is critical that such toxic and very long-lived isotopes should be kept isolated from the environment until their radioactivity has decayed to a harmless level.

The problem is also exacerbated by the fact that the US nuclear power industry is likely to grow at such a rate between 1980 and the turn of the cen-

tury that its production of nuclear waste will increase by a factor of about eight.

At present, the AEC operates three reprocessing plants, at Richland in Washington, Savannah River in South Carolina, and in Idaho. There is also a commercially operated plant in New York and several others are on the way. Past practice has been to store the radioactive wastes in solution, but the AEC now requires that to prevent the likelihood of spillage and to cut down the volume of waste to be disposed of, radioactive waste must be solidified (calcined) before it is delivered to the commission for long-term storage. The AEC expects the first batches of calcined wastes to arrive from commercially operated fuel reprocessing plants in 1979 or 1980.

The commission already has some experience of storing calcined wastes in its Idaho plant, where for the past ten years such wastes have been stored in stainless steel and concrete bins in underground bunkers. But the AEC has always regarded such storage facilities as a temporary expedient, until more permanent salt-mine storage can be brought into use. The beauty of the concept of storing hot, radioactive wastes in salt deposits is that not only can the salt heal its own fractures by plastic flow, but that it is also a good heat transmitter which provides about the same radioactive shielding as concrete.

The basic idea is to scrape a hole in the bottom of a disused salt mine, lower the calcined cannisters of waste material into it and then to fill the cavern with crushed salt. Because the calcined waste would generate heat by radioactive decay—the temperature in the centre of the cannister would be about 950° C—the crushed salt would be recrystallized and this would flow to seal off the buried cannisters. Provided the salt mine were not subject to geological disturbance and that it would be free from ground water flow, the calcined material would be sealed off from the rest of the environment, even if the cannisters eventually broke.

Until last year, the AEC believed that it had found just such a salt mine near the town of Lyons in Kansas, but a series of alarming occurrences caused it to change its mind and in October it abandoned all site work at the mine. One problem is that the mine is punctured by thirty bore holes, three of which cannot be completely plugged, but the chief factor which forced the AEC to turn its back on Lyons and

search for other salt mines came to light during mining in a neighbouring deposit owned by the American Salt Mine Co. The company was extracting salt from the mine by solution mining—pumping fresh water into the shaft and extracting the brine for processing—when it lost 180,000 gallons of fresh water. The water presumably percolated into the underlying rock strata, suggesting that, as William Hambleton, Director of the Kansas Geological Survey puts it, the area is like a piece of Swiss cheese with great potential for entrance and circulation of fluids. Such a network would, of course, seriously endanger the chances of storing radioactive wastes in isolation from the rest of the environment.

The commission has therefore cast its net wider and according to one source, has eight other areas of Kansas under study, together with a potash mine in New Mexico, and is also considering the possibility of salt domes on the Gulf Coast. One of these sites will probably be chosen for the pilot study announced by the AEC last week, and presumably if the study is encouraging, salt-mine disposal will eventually take over from the surface bunkers now planned.

A committee of the National Academy of Sciences reported in 1966 that salt-mine storage seems to offer the best method for long-term disposal of highly radioactive wastes. But the committee has always insisted that other methods are studied so that, if the salt mine concept does not live up to expectation, the commission will not be left with undisposible radioactive wastes on its hands.

Two such concepts have been given public airing during the past year or so. One, proposed by scientists from the Lawrence Livermore Laboratory, entails creating an underground cavity filled with broken rock by a small nuclear explosion and using this cavity to store highly radioactive solutions of nuclear wastes. The suggestion is that radioactive decay in the solutions will boil them dry, and eventually, when the cavity's capacity is reached, the heat generated would melt the rocks in and around the cavity. The rock would then dissolve the waste and eventually the whole mass would solidify into an insoluble permanent entrapment. The other prominent suggestion, aired by Dr James Schlesinger, chairman of the AEC in an interview with a weekly news magazine, would involve disposing of the wastes by loading them on to a rocket and shooting them into the sun.

Whatever method is finally chosen

for waste disposal, it will have to be sufficient to process up to about 70,000 cubic feet of calcined material a year by the turn of the century. The Atomic Energy Commission estimates that by that time, about 900,000 cubic feet of waste will have been produced by the nuclear power industry in the United States.

HIGH ENERGY PHYSICS

Fat Free Diet

by our Washington Correspondent

IF high energy physicists were looking to the Joint Committee on Atomic Energy to provide salvation from the financial ills that have plagued them for the past few years, they have been badly disappointed. The joint committee, far from being a saviour, has not only recommended that \$2 million be cut from the AEC's budget request for high energy physics next year, but also has disquieting things to say about future funding.

If that were not bitterness enough for high energy physicists, the joint committee has given the AEC its traditional vote of confidence by recommending budget increases in many other areas. Each year the committee picks over the AEC's budget request and recommends upper spending levels, while the appropriations committees in the House of Representatives and in the Senate recommend the actual amounts of money that Congress should make available. And each year, the committee votes to give the AEC more money than it asked for. Few other government agencies can boast of such a cosy relationship with their Congressional watchdogs.

This year the joint committee has recommended that Congress should provide the commission with about \$2,710 million in the 1973 fiscal year—some \$61 million more than it requested. Chief beneficiaries of the joint committee's largesse are nuclear materials technology, civilian reactor development—particularly work on the fast breeder reactor—Plowshare and space nuclear propulsion.

What the joint committee recommends for high energy physics is that the AEC should get the \$126.4 million it requested to run its accelerators but that it should be denied \$500,000 of what it had requested for improving accelerators and \$1.5 million of the funds earmarked for capital equipment. These reductions would pare the budget for plant and capital equipment for high energy physics from \$30.5 million to \$28.5 million.

It is true that, even with the \$2 million reduction, if the AEC gets all the money authorized by the joint committee for high energy physics, that will halt and even reverse the decline of

the past few years. But past financial stringencies have bitten so deeply that, according to some AEC officials and most of the high energy physicists who testified during hearings on the AEC's budget earlier this year, the high energy physics programme has been pushed to the edge of viability. As a consequence, they claim that some vital areas such as accelerator research and development and accelerator improvements have suffered badly, and they are alarmed at the suggestion that accelerator improvements should be put off.

The joint committee's argument is that since the commission's six accelerators are all being run below peak capacity, it would be a misallocation of resources to spend a great deal of money on improvements, and that "a lower ratio of equipment and construction funds to operating funds would be more realistic". There are, of course, two routes by which the ratio can be lowered, and the committee chose the cheaper—reducing the capital funds rather than increasing the operating budgets.

The joint committee has also cast a shadow over the future. Although acknowledging the warning it has been given that shutting down either of the two older accelerators (the Bevatron at the Lawrence Berkley Laboratory or the Zero Gradient Synchrotron at Argonne National Laboratory) would cause irreparable damage to university and college high energy physics programmes, the joint committee is plainly in two minds about the desirability of spreading limited resources over several machines. Specifically, the committee points to a study by the General Accounting Office which suggests that, at 65 to 80 per cent of optimal operating funding, accelerators can be operated for only about 50 per cent of their available time. This conclusion, the committee suggests, "would seem to support a program policy of optimum funding for operation of fewer accelerators rather than less than optimum funding . . . of all existing facilities". The joint committee has therefore asked the AEC to provide Congress with a report recommending appropriate changes in funding "to assure improved efficiency in utilization of the high energy physics accelerator facilities while providing for facility improvements commensurate with high utilization". The AEC has, however, already pointed out to the joint committee that it would not sanction the shutting down of more high energy facilities (see *Nature* **236**, 52; 1972).

Other funding recommendations made by the joint committee include \$2 million for the gas-cooled fast breeder reactor development programme, but since \$131.5 million is being provided for the base technology programme for the liquid-metal fast-

breeder concept, the work on the gas reactor is simply designed to provide an alternative approach to breeder reactor design; the committee has authorized expenditure of \$56.8 million for the AEC's reactor safety programme, which has recently been the subject of a public enquiry and much public anxiety, an increase of \$3.5 million above the commission's budget request; the joint committee has added \$7 million to the commission's request for \$5 million to develop a small nuclear powered space propulsion system, chiefly to ensure that not all of the \$1,400 million spent so far on development of the large NERVA rocket will be wasted. The committee has reluctantly acquiesced in the AEC's decision to terminate the NERVA project; the AEC's request for \$38 million for operating expenses for the controlled thermonuclear fusion programme has been agreed to, but the committee has instructed the General Accounting Office to conduct a study of the management procedures employed in AEC-supported facilities operating the programme; and, finally, the committee has added \$3.7 million to the AEC's request for \$10.5 million for the Plowshare programme, which is designed to excavate natural gas by use of nuclear explosives.

Short Notes

Invitations to Jupiter/Saturn

THE National Aeronautic and Space Administration, which substituted a Mariner Saturn/Jupiter flyby mission for the Grand Tour, is now inviting proposals from scientists to participate in the mission. The agency expects to distribute detailed information about the mission early in June, and proposals will be due in by August 31, 1972. Interested scientists should send names and addresses of potential proposers and the specific area of investigation to M. A. Mitz, Program Scientist, Code S. L., Outer Planets Missions, Office of Space Science, NASA Headquarters, Washington DC 20546.

Drug Abuse

THE Drug Abuse Council, a newly established organization funded by the Ford Foundation, the Carnegie Corporation, the Commonwealth Fund and the Henry Kaiser Family Foundation, has awarded its first grants. The grants, which amount to \$425,000, are aimed at encouraging scientific research in drug abuse and for drug education. The largest package of grants, amounting to \$300,000, will be used to set up fellowships which will be open to individuals with a proven record in research. As many as 15 will eventually be awarded.

Nuclear Structure Facility : Ten Years On

THE construction of an electrostatic accelerator capable of generating a potential of 20 million volts (MV) is now feasible, according to a design study team which has been looking at the viability of such a machine for the past fourteen months. Dr R. G. P. Voss, deputy director of Daresbury Nuclear Physics Laboratory and leader of the team, said last week that a favourable report would be made to the Science Research Council in August when the contract for the design study expires.

Dr Voss thinks that building the accelerator will cost £4 million over a five year period. This sum will include a building to house the accelerator and also some equipment to start the experimental programme. The cost of the design study has been between £300,000 and £400,000.

The accelerator—a tandem Van de Graaff—will, it is hoped, eventually produce a voltage of 30 MV on its centre terminal, although several technical developments will have to be made before the highest voltage is achieved. There are no comparable machines available and the nearest in performance are manufactured by the High Voltage Engineering Corporation (HVEC) of Burlington, Massachusetts, and give 15 MV.

If the Science Research Council decides to allocate money for the accelerator, it will end a decade of frustration for nuclear structure physicists in Britain. Since the early 1960s political squabbles over where the accelerator should be sited, together with lean times for the finance of nuclear physics, have delayed decisions on this new generation of low energy nuclear physics accelerators.

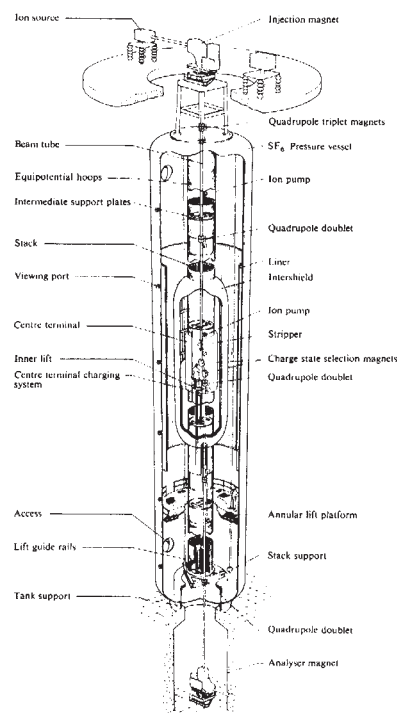
Even now it has not been decided where the accelerator is to be sited. The Nuclear Physics Board of the SRC has recommended that it be built at Daresbury but this decision has to be ratified by the council. The signs are, however, that the SRC will not ignore this advice.

The successful outcome of the design study is a feather in the cap of Dr Voss. When the study was initiated a year ago, it was far from clear whether the study team should be investigating the viability of an electrostatic accelerator or whether it should have been devoting its attention to the possibility of building a superconducting accelerator for heavy particles. Time has shown, however, that the chosen course was the correct one, for progress on the electron superconducting accelerator now being built at Stanford University is painfully slow and several fundamental prob-

lems remain to be solved.

Dr Voss's plans for the Van de Graaff accelerator pander to the tastes of the people who favour a superconducting accelerator, because the building is designed so that a linear accelerator of this kind could be added at the end of the Van de Graaff at some future date.

The proposed accelerator will be built vertically—in contrast to the HVEC machines which are all positioned horizontally. The accelerator pressure tank will be 41.3 m high and more than 8 m in diameter, and will be incorporated in a tower 63.5 m high (see Figure).



It is planned that the accelerator will be operated in an atmosphere of pure sulphur hexafluoride held at a pressure of several atmospheres to decrease the possibility of electrical breakdown through the gas. One of the chief problems associated with the construction of an accelerator operating at such high voltages is the building of parts capable of withstanding the electrical surges caused by sudden discharges of high voltages.

Dr Voss's confidence that the accelerator can now be built is based on the work that the design study team has carried out on the electrical properties of those components that will be placed at high potentials within the accelerator;

in particular the supporting pillars, or stack, of the accelerator have been specially designed with this in mind. More important, the design is such that the individual component parts of the stack can be replaced if they do fail.

Another innovation is the system for carrying charge to the high voltage terminal of the accelerator. In most machines of this type, charge is transferred to a continuous rubberized fabric belt wound around two pulleys. The shortcomings of this system are that the transfer of charge is irregular—resulting in a voltage ripple—and that the belt tends to disintegrate and produce dust within the accelerator when breakdown occurs.

The first improvements in the method of carrying charge were suggested a few years ago by Professor R. Herb, of the University of Wisconsin, and eventually incorporated by him in electrostatic accelerators built for the University of São Paulo and the Australian National University. The fabric belt is replaced by a series of metal beads connected by an insulating string. This system—called a pelletron—has now been further developed by the Daresbury team into a system dubbed the laddertron, which has a much greater current carrying capacity than either the conventional belt or the pelletron and has the advantage of greater mechanical rigidity.

Professor Alick Ashmore, director of Daresbury Nuclear Physics Laboratory, said last week that even if the Nuclear Structure Facility is built at Daresbury the chief function of the laboratory would still be to carry out research in high energy physics. Professor Ashmore pointed out that the future of high energy physics within Britain after 1976 is to some extent undecided and that once the 300 GeV accelerator at CERN goes into operation, decisions would have to be made about the future of the two British national accelerators—Nina at Daresbury and Nimrod at the Rutherford Laboratory. Britain cannot both keep two accelerators of this size fully operational and maintain a full programme at CERN, chiefly because of the lack of sufficient qualified physicists.

One of Professor Ashmore's concerns is that if the SRC gives the go-ahead to the Nuclear Structure Facility, design and engineering support would principally come from the present staff at Daresbury and the high energy work would suffer as a consequence. Even so, he said that he would welcome the project enthusiastically if it is decided to house it at Daresbury.

NEWS AND VIEWS

Radical View of Anomalous Redshifts

THE proposal made on page 227 of this issue of *Nature* by Pecker, Roberts and Vigier for an explanation of anomalous redshifts by means of mutual interactions between photons has radical implications.

Students of special relativity are always struck by the prominence which seems to be given in elementary accounts to the velocity of light. This prominence is a delusion; rather the union of space and time in Einstein's theory exhibits the existence of a fundamental velocity not exceeded by massive particles. The hypothesis that this fundamental velocity is always attained by light signals is an independent statement; but it is very elegant and it allows a satisfactory formulation of electrodynamic equations. Furthermore, it makes the fundamental velocity predicted by special relativity amenable to experiment, and it also explains why photons are never observed to be at rest.

To deny this statement and to hypothesize instead that light particles which have a mass not exactly zero are capable of velocities less than the fundamental velocity is to make a very bold and radical proposition. The mutual interaction between photons proposed by Pecker, Roberts and Vigier has this radical character. The fact that their hypothesis is a good one because it can be tested and also because it explains several anomalies will therefore be seen as puzzling.

The need to formulate a hypothesis along these lines is a result of the existence of anomalous redshifts in astronomical observations. Redshift (that is, systematic lengthening of wavelengths of the components of a known spectrum, like playing a familiar tune flat) is one of the keys to understanding of the structure both of massive objects and of the universe on a large scale. Geometrical and kinematical explanations rather than interactions are usually invoked to explain its occurrence. The gravitational redshift is a geometrical explanation (in terms of the curved geometry of general relativity) of the reddening of light from massive stars with known spectral composition, including the Sun, and the recession of galaxies in the expanding universe is the conventional kinematic explanation of the redshift of light from very distant objects.

But these conventional explanations leave several phenomena unexplained. The most conspicuous of these are the redshifts from quasi-stellar objects whose small size makes the conventional explanations inapplicable and has led people to look for other means of producing large redshifts—but this problem is not directly relevant to the present discussion. Two other such phenomena are small discrepancies in the redshift of light from the star whose gravitational field is known with best precision—the Sun—and redshift anomalies in the light from certain galaxies.

The proposed explanation is that by mutual interaction between photons, a photon travelling through a "cloud" of other photons could lose energy and be reddened. This

interaction has the undesirable features noted earlier; but it explains in a striking way the anomalous galactic redshifts as being the simple result of variations of temperature between one galaxy and another, which affect the density of the photon cloud. The interaction constants associated with the proposed interaction can be determined for the extent of the discrepancy in the solar redshift observations.

Pecker *et al.*'s article also contains two more surprises. First, an anomaly in the currently accepted distribution of the pulsating stars in our galaxy, which was obtained by assuming that their distance could be measured by noting the differential arrival times of different frequencies of radiation, is explained if it is supposed that this radiation (whose spectral form does not permit direct redshift observations) was subject to the interaction proposed near to the stellar surface; second, the redshifts of very distant objects themselves—the evidence for the expanding universe and the whole modern science of cosmology—can be explained by scattering of photons as they travel through the universe's background radiation—calculation of the background temperature on that assumption then leads to the currently accepted value.

The suggested effect is amenable to laboratory test. If it works, some tiny discrepancies will be resolved, cosmology will be shattered and electrodynamics will be in disarray. Relativists will await experimental results with interest.—P. E. R.

Poor Man's Rocket

If the rocket had not been invented, the study of meteors would by now be one of the chief means for examining the upper atmosphere. Even as things are, scientists observing meteors have the edge when rockets would be too expensive to provide coverage of rapidly altering phenomena between heights of about 80 km and 110 km, where the atmospheric friction on an incoming meteoric particle becomes so severe that most of the particles burn up. The prime example is the measurement of wind velocities in this region of the atmosphere, which is possible by tracking the drift of the trails of ionization left behind by the destroyed particles using equipment which is a direct descendant of the radar sets of the Second World War.

Over the years a pattern of the ebb and flow of this region of the atmosphere has been discerned by this method, which is probably a more reliable indicator of atmospheric motions than the complementary technique of observing the motion of irregularities in the ionospheric layers, and the behaviour of the pattern at a handful of stations has been examined on time scales of months down to tens of minutes.

By giving the wind pattern in a detail that would be out of the question from observations of smoke trails from rockets, the radar observations were one of the factors which led to the introduction in the early 1960s of the gravity-wave interpretation of many atmospheric phenomena. Indeed, with growing numbers of atmospheric observers leaning towards the view that the links between the surface layers of the atmosphere and the higher regions may be less tenuous than used to be suspected, there is still plenty for the radar observers to do in portraying the wind motions in this narrow but possibly crucial layer of the atmosphere.

A further example of the way in which radar observations of the ionized trails left behind by meteoric particles may give information about the atmosphere in which the particles burn up—in addition, of course, to information on the astronomical nature and origin of the particles themselves—is set out on page 224 of this issue of *Nature*. D. W. Hughes of the Sheffield meteor group and W. J. Baggaley, formerly of Sheffield but now at Canterbury University, New Zealand, describe observations of the Quadrantid meteor shower which at Sheffield occurs near dawn on January 4 each year when the Earth passes through a concentrated stream of dust particles presumably left behind by a disintegrating comet, the number of meteors for several hours during the shower being many times higher than on a normal day when no showers are active.

On previous occasions the Sheffield group have noticed an increase in the number of longer duration meteor echoes lasting more than a few seconds from the Quadrantid shower at about the time when the Sun was rising on the region high in the atmosphere where the Quadrantid meteors burn up. Hughes and Baggaley set out to discover the reason for this effect from detailed radar observations of this year's recurrence of the Quadrantid shower.

They have identified the increase in the rate of occurrence of longer duration echoes as taking place about an hour after sunrise on the echoing layer, and discuss whether the duration of the echoes could have been increased by an increase in atmospheric turbulence associated with heating of the atmosphere at sunrise—which would lead to an increase in

the number of meteor trails which are at the correct orientation to give a radar echo—or whether there is some change at sunrise in the chemical constituents of the atmosphere which control the loss of ionization from the longer-lasting trails.

Hughes and Baggaley conclude that to some extent both factors are at work. There is a post-sunrise increase in the number of echoes which are up to 8 s long because of changes in the wind pattern, but the post-sunrise excess of echoes with longer durations probably has to do with the chemistry of the region, there being a change in the processes which govern the loss of electrons from the ionized trails.—By our Astronomy Correspondent.

MARINE BIOLOGY

Research in Jamaica

from a Correspondent

CORAL reefs are the most complex of marine ecosystems. Their investigation has been held back by lack of adequately equipped laboratories from which, as in temperate water marine stations, long term observations—such as determinations of breeding seasons and life histories—can be conducted and where physiological work can be carried out in the closest contact with ecological studies.

An important advance in the filling of such a need was made with the opening in March 1970 of the Discovery Bay Laboratory on the north coast of Jamaica, some 40 miles east of Montego Bay. For its first year of operation the laboratory was run by the station manager, Mr N. Copland, but from the beginning of this year it has had a full-time director, Mr P. M. J. Woodhead, who was formerly on the staff of the Fisheries Laboratory at Lowestoft and then for three years was in charge of the Heron Island Laboratory at the

southern end of the Great Barrier Reef. It is expected that the laboratory (address: Box 36, Discovery Bay, Jamaica, WI) will soon become a leading centre for research in tropical marine biology, with particular reference to Atlantic coral reefs.

The laboratory, which replaced the very simple facilities which were previously available, lies off the edge of reefs which are now among the best known in the world, largely through the efforts during the previous twenty years of the late Professor T. F. Goreau and his colleagues. Professor Goreau just lived to see the opening of the laboratory, which represents a most suitable memorial to this outstandingly original and productive worker.

The laboratory is equipped with all the usual facilities associated with marine stations. On the left-hand side of the photograph can be seen the "wet laboratory" which has a central water table and a row of aquarium tanks provided with continuous seawater circulation. There is space here for more than twenty students or research workers. On the right of the main entrance is a series of six research laboratories with a total floor area of 2,100 square feet, together with an instrument room, a dark room and offices. The equipment includes a wide range of SCUBA diving apparatus—including air tanks, a compressor and a small recompression chamber—a 22-foot fibreglass boat and a 15-foot plywood boat.

The cost of erecting the laboratory was met, approximately half and half, by grants from the Wolfson Foundation and from the University of the West Indies which has also been responsible for the usual final modifications. Salaries are met by the Marine Sciences Research Center of the State University of New York. Final responsibility for the running of the laboratory is in the hands of a committee of management consisting of two members from the University of the West Indies (with Professor I. Goodbody as chairman), two from the State University of New York, and Sir Maurice Yonge from Edinburgh.



Discovery Bay Laboratory, Jamaica.

WHEAT

Contested Parentage

from a Correspondent

NEW interest in the evolutionary origins of the common wheat of agriculture, *Triticum aestivum*, has been stimulated by the discovery that some of its diploid relatives, in the genus *Aegilops*, contain genetic variation which affects meiotic chromosome pairing. *T. aestivum* is a hexaploid which contains the complete sets of chromosomes—known respectively as the A, B and D genomes—of three distinct diploid species. It has long been accepted that the A and D genomes were derived respectively from close ancestors of the contemporary diploids *Triticum monococcum* and *Aegilops squarrosa*. More uncertainty surrounds the origin of the B genome.

It should first be pointed out that in each genome of wheat the chromosomes relate genetically to corresponding, or homoeologous, chromosomes in each of the other genomes. Homoeologous chromosomes do not normally pair at meiosis, being prevented from doing so (Wall, Riley, Chapman and Gale, *Genet. Res.*, **18**, 311 and 329; 1971) by the activity of a single locus (*Ph*) which is distantly located on the long arm of chromosome 5B. The genotypes of *speloides* and *mutica* have the capacity, in hybrids with *T. aestivum*, to suppress the activity of the *Ph* locus. Dover and Riley (*Nature New Biology*, **235**, 61; 1972) and Kimber and Athwal (*Proc. US Nat. Acad. Sci.*, **69**, 912; 1972) have now described the existence of genetic variation which affects the occurrence of homoeologous meiotic pairing in hybrids between *T. aestivum* and *Ae. mutica* and *Ae. speloides* respectively.

Ae. speloides, for reasons based on karyotype, gross plant morphology and geographical distribution, has been considered to be a strong candidate for recognition as the donor of the B genome. Kimber and Athwal now interpret the patterns of meiotic chromosome pairing observed in wheat-*Ae. speloides* combinations, in which the *Ph* activity is apparently not suppressed, as implying no closer relationship between wheat and *Ae. speloides* than that between wheat and many other *Aegilops* species, so reducing the likelihood of *Ae. speloides* being the source of the B genome. The essence of Kimber and Athwal's argument is that, even if homoeologous pairing cannot occur, there should still be pairing between chromosomes of the B genome and their full homologues in the *Ae. speloides* genome.

The strength of this case will ultimately depend on the cause of the low-pairing behaviour in the critical wheat-*Ae. speloides* hybrids. It might be the result merely of a form of asynapsis, and this could be tested by making

hybrids in which one wheat chromosome was disomic as a result of the cross *T. aestivum* tetrasomic for one chromosome $\times$ *Ae. speloides*. If Kimber and Athwal are correct in their interpretation, the disomic chromosomes should pair normally, pairing being prevented only between homoeologues. Conclusions, based on other criteria, that *Ae. speloides* was involved in the parentage of wheat would then be more deeply undermined.

Another question that might be resolved using the newly recognized pairing variants, as proposed by Dover and Riley, is the nature of the origin of the *Ph* allele of *T. aestivum*. Because no activity similar to that of *Ph* is known among the diploid relatives of wheat, it has long been thought that this allele must have arisen by mutation after chromosome 5B had been incorporated in polyploid wheat. The genotypes of *Ae. speloides* and *Ae. mutica* that, in hybrids with wheat, result in low levels of meiotic pairing, may, however, carry alleles corresponding to that on chromosome 5B, in which case, as Dover and Riley suggest, there would be low pairing in similar hybrids lacking chromosome 5B because the presence of the diploid genome would compensate for its absence. This question, however, has still to be resolved.

The course of evolution of wheat is still a controversial issue. Clearly, difficulty is created by attempts to demonstrate parentage experimentally, with contemporary material, several

thousands of generations of evolution subsequent to the hybridization event and there will always be uncertainty. Crop plant evolutionists now, however, have a better specification of the characteristics of the B genome donor than formerly, when such bizarre proposals were made as that the B genome came from *Agropyron triticeum*, in spite of the fact that all the chromosomes of this species have subterminal centromeres and those of the B genome are median or submedian.

VIROLOGY

Captive Genomes

from our Cell Biology Correspondent

REOVIRUS places several claims for attention on virologists. For one thing, its genome is unusual; it comprises ten segments of double stranded RNA. For another, the virus particles contain an RNA transcriptase which, once the outer of the two capsids has been removed *in vitro* by proteolytic enzymes, will transcribe one of the two strands of each of the ten double stranded segments of the genome, all of which are themselves conserved.

These so-called sub-viral particles are an interesting source of messenger RNAs, and their properties at least raise the possibility that *in vivo* during the infectious cycle the reovirus genome, unlike the genomes of most

Selective Inhibition of RNA Synthesis

INFORMATION about new drugs or new ways to exploit tried and tested drugs, which allow experimentalists to probe further into the metabolic processes of eukaryotic cells, is always welcome and no doubt the suppliers of camptothecin can look forward to a boom in demand as a result of what Abelson and Penman have to say in *Nature New Biology* next Wednesday (May 31). They show that camptothecin, which is known to induce breakages in cellular DNA, also selectively interrupts the synthesis of high molecular weight RNAs in HeLa cell nuclei.

Abelson and Penman have confirmed that when HeLa cells, growing in suspension, are exposed to 1 μ g/ml. of the drug, DNA and RNA synthesis are both markedly inhibited whereas protein synthesis is hardly affected. Closer analysis of the small amount of RNA made in the presence of the drug reveals its selective effect. Although the average size of the molecules in ribosomal RNA precursor and heterogeneous nuclear RNA fractions is very much smaller than in controls the synthesis of 4S RNA is only slightly

impaired and 5S RNA synthesis seems to be completely unaffected by the drug at this and higher doses. Furthermore, whereas the truncated ribosomal RNA fractions made in the presence of the drug are rapidly degraded within the nucleus, the aberrant heterogeneous nuclear RNA is comparatively stable; it persists in the nucleus, being neither degraded nor exported to the cytoplasm.

How, at the molecular level, camptothecin brings about these changes remains to be elucidated; it is not apparently simply a matter of slowing down the rate of growth of RNA chains, and whether or not the drug inhibits RNA synthesis because it causes breaks in the template DNA is as yet an open question.

Abelson and Penman confirm one thing, however; camptothecin can be used to inhibit selectively synthesis of ribosomal and messenger RNAs without concomitantly blocking the synthesis of 4S and 5S RNAs or RNA synthesis in mitochondria. It will therefore increasingly find a place on the laboratory shelf.

other viruses which are liberated from the virion in order that they may replicate, remains enclosed in a sub-viral particle similar if not identical to the sub-viral particles produced *in vitro* by exposing virions to proteolytic enzymes. That this is indeed the case has now been established by two groups; Chang and Zweenik (*Virology*, **46**, 544; 1971) and Silverstein *et al.* (*ibid.*, **47**, 797; 1972).

From the combined data obtained by these two groups and others the following picture of the reovirus infectious cycle emerges. The reovirus particle enters a cell in a phagocytic vacuole which promptly fuses with a lysosome within which the outer capsid of the virion is digested to leave a subviral particle. Analysis of the polypeptides in such subviral particles produced *in vivo* reveals that at least one polypeptide derived by cleavage from a protein in the outer capsid remains associated with the inner capsid, or core polypeptides enclosing the viral genome and that the *in vivo* sub-viral particles are not, therefore, identical to those produced *in vitro*. Nevertheless, like the *in vitro* sub-viral particles, those isolated from infected cells continue after extraction to synthesize single stranded RNA molecules which fall into three size classes, corresponding to the three classes of genomic RNA. Hybridization studies reveal, first, that the single stranded RNAs are the products of asymmetric transcription and, second, that among the total population of single stranded RNAs are transcripts of all ten segments of the genome.

In the infected cell the parental sub-viral particles seem to remain intact throughout the replication cycle and the single stranded RNAs are exported from the particles to the cytoplasm where they act as messengers. These single stranded RNAs also apparently act as templates for the replication of complementary chains to yield progeny genomes, and because little or no free double stranded RNA can be isolated from infected cells this replication presumably also occurs in a sub-viral particle. Finally, at late times in the infectious cycle newly made outer capsid proteins associate with the parental sub-viral particles as well as the progeny sub-viral particles. In short, as Silverstein *et al.* comment, sub-viral particles "are the structural and functional units of reovirus infection"; the infecting genome is never completely liberated from the structural proteins of the virion.

Like reovirus the syncytium forming or foamy viruses are also currently attracting considerable attention, but for very different reasons. These cytopathic and ubiquitous single stranded RNA viruses contain an RNA dependent DNA polymerase, or reverse

transcriptase, similar to that present in the RNA tumour viruses. The mere presence of this enzyme in foamy virus particles suggests that their replication involves a DNA intermediate. Consonant with that idea, Parks *et al.* reported that actinomycin D greatly reduces the infectivity of simian foamy virus.

Now Parks and Todaro (*ibid.*, **47**, 673; 1972) have devised an 8 day plaque assay for bovine, feline and simian foamy viruses, have determined the growth cycle of simian foamy virus in human fibroblasts, and have shown that the thymidine analogue BudR at low concentrations markedly reduces the yield of infectious virus obtained when cells are infected with foamy viruses. This result also strongly implicates a DNA molecule in the replication of these viruses, which also share with the RNA tumour viruses a high resistance to inactivation by ultraviolet light.

The foamy viruses do not, however, seem to be oncogenic; all of the various cell types which Parks and Todaro have infected with one or other of these viruses have proved to be permissive for virus replication and have not assumed a transformed-cell phenotype. All of which goes to prove that the occurrence of reverse transcriptase in a single stranded RNA virus particle is not an infallible indication of an oncogenic potential.

Origin of Multimeric ϕ X174 DNA

THE small genomes of many single stranded and double stranded DNA bacteriophages and viruses are covalently closed circular molecules. It has frequently been observed that when such genomes replicate in host cells a small proportion of the progeny molecules are twice as long as the parental molecules and even smaller proportions of the progeny are higher multiples of the parental genome.

There are two possible ways in which such multimeric DNA molecules might be generated—by recombination or by some failure to terminate replication—and evidence for one or other mechanism has been obtained by workers investigating the replication of various phages and viruses. Bacteriophage ϕ X174 is the most familiar of the coliphages with a circular single stranded DNA genome, and according to Benbow, Eisenberg and Sinsheimer's report in *Nature New Biology* next Wednesday (May 31) most circular multimeric ϕ X174 DNA molecules arise not by recombination but as a result of some failure in the replication of the DNA.

Benbow *et al.* find, for example, that circular dimers and multimers of ϕ X174 DNA are generated when the phage replicates in mutant *Escherichia coli*

PROTEINS

Vistas in Viscosity

from our Molecular Biology Correspondent

WITH only a capillary viscometer and a computer program or two, Harrington and Burke (*Biochemistry*, **11**, 1448; 1972) have deduced an explicit geometry for the myosin dimer, that exists in solution in rapid equilibrium with the monomer. This is more than a mere assertion of the enduring virtues of classical hydrodynamic method when the circumstances are favourable, for the myosin dimer is progressively favoured as the ionic strength decreases to the point at which filaments begin to form. It thus seems highly likely that it represents the first step in the process of thick-filament assembly. The structural evidence of Huxley indicates that the thick filaments contain parallel-packed myosin molecules, pointing in either direction from a central region in which the shafts meet, and there are no heads to form cross-bridges.

Earlier work from the same laboratory having established that the solutions of myosin contain only the monomer and dimer in rapid equilibrium, Harrington and Burke have demonstrated that this is also the case for myosin rods, bereft of heads after papain treatment, and for light mero-myosin, which is a proteolytically

cells in which recombination is defective. Moreover, when a cell is co-infected with two mutant strains of ϕ X174, the DNA of which differs significantly in length, the small amount of circular dimeric and multimeric DNAs that arise fall into two classes, each class containing molecules that are exact multiples of the two parental DNA molecules.

Few if any of the multimeric circular molecules have lengths expected of molecules that comprise mixtures of the two parental DNAs. On the other hand, catenated molecules—forms in which two circular monomers are interlinked like the links of a chain—are generated in infected cells in which phage DNA replication is blocked. In doubly infected cells such catenated molecules comprise interlocked DNAs of the two parental strains.

In short, although Benbow *et al.* have no direct evidence about the precise molecular mechanisms involved, it seems clear that circular multimeric ϕ X174 DNA molecules are generated by some failure in replication and not by a recombination event. As they caution, however, these findings pertain only to ϕ X174 and should not be extrapolated to other systems.

truncated rod. The sedimentation equilibrium profiles, expressed in terms of molecular weight distribution down the liquid column, are in each case characteristic of a monomer-dimer system, and give the limiting molecular weights and the association constant. The free energies of association for all three species, light meromyosin, whole rods and intact myosin, are practically identical, which gives credence to the notion that they all associate in exactly the same manner. The viscosity measurements extend to higher dilution than any attempted by earlier workers, and it is for this reason that the downward curvature in the concentration extrapolation, indicative of the association-dissociation equilibrium, has only now been observed.

The shape of the curve depends in a predictable manner on the intrinsic viscosities of the two species, the concentration-dependence of the reduced viscosity of either, and the association constant. All the parameters can be easily enough extracted, and the association constants agree well enough with those from sedimentation. Now the intrinsic viscosity is dominated entirely by the asymmetry of the molecule, and that of the dimer being bigger than the monomer, it follows that its axial ratio is also greater. Furthermore, the lengths of the rod and of light meromyosin are known, and granting that the mode of association of the two is the same, the number of ways of assembling a dimer, which will be longer than the corresponding monomer, and which can form from either the intact or the truncated rod, is limited.

Viscosity theory for rigid rods gives a very fair estimate of the length of the dimer, which gives in turn the amount of overlap of the two constituent rods. The structure that is deduced is thus that of a parallel association, with 430–530 Å overlap. The expected dependence of viscosity on overlap is not small: a parallel dimer with complete overlap should give just half the observed value. Recently Herbert and Carlson extracted from their measurements of wavelength distribution of scattered laser light from myosin solutions a length and radius of gyration for the dimer, both of which came out gratifyingly close to the values reduced from viscosity.

In an accompanying article (*ibid.*, 1456) Burke and Harrington break new ground in exploring the limitations of theory for rotational frictional properties of rods. Using brass models and a device for measuring torque, they find remarkable agreement of the measured frictional coefficient with theoretical prediction. Sustained by the warm glow of confidence thus generated, they go on to calculate the viscosity for

dimers made up of parallel rods with varying extents of overlap, as well as for dimeric intact myosin, making allowance for the globular heads. The sensitivity of the viscosity to the length is critical, and the results are consistent with measured values for a 430 Å overlap of parallel dimers, this arrangement applying equally to intact rods, light meromyosin and whole myosin.

Bode, Engel and Winklemair (*Europ. J. Biochem.*, **26**, 313; 1972) have tackled another fibrous assembly, the bacterial flagellum, starting with the monomer. This is a smaller protein, with molecular weight 40,000, but there seems no doubt that it has considerable asymmetry. Bode *et al.* find in the first place that its Stokes radius is larger than that of globular proteins in terms of its elution from 'Sephadex' columns, and its hydrodynamic frictional coefficient is likewise high. From the viscosity an estimate of the equivalent ellipsoid dimensions can be made—though with

an axial ratio apparently of the order of 10 this is not a quantitative proposition, in the same sense as for the much more asymmetric myosin, because at this level the hydration still contributes a significant and somewhat uncertain proportion of the viscosity.

Low-angle X-ray scattering is next used to give a radius of gyration, and Bode *et al.* find that the scattering envelopes can be remarkably well fitted on the basis of a wedge-shaped model. An ellipsoid, on the other hand, is less satisfactory. The notion of a wedge shape comes in fact from the tight packing of the subunits in the flagellum, as seen in the electron microscope. As in TMV, this seems to require that the subunits be broader at the outside than the centre of the filament. This kind of packing with wedges radiating outwards at an angle to the axis may be related also to the V-shaped ends of sonicated flagella. Given also a small stain-penetrable tunnel up the middle, Bode *et al.* are able to arrive at an explicit

Reduction of Conductivity of NaCl

IN next Monday's *Nature Physical Science* (May 29), W. Whitham and J. H. Calderwood of the University of Salford report that they have succeeded in almost completely eliminating the extrinsic (impurity induced) electrical conductivity of some samples of sodium chloride. What they have done is to add a little boric oxide (B_2O_3) to a sodium chloride melt before growing crystals from it by the usual Czochralski method.

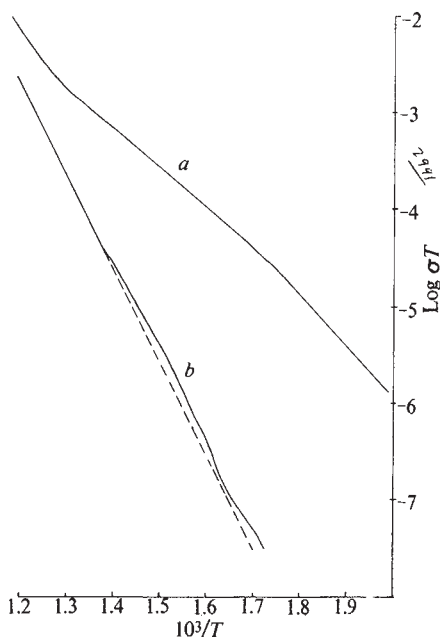
The lower curve in the diagram here shows the result of adding the oxide to a melt made up of BDH 'Analar' sodium chloride. The upper curve is the conductivity of a crystal of NaCl grown in the absence of boric oxide; the lower curve is more or less an extrapolation of the intrinsic (high temperature) region of the upper curve. Whitham and Calderwood found that the lower curve became almost exactly a straight line when measurements were made on a heat treated sample.

Whitham and Calderwood suggest that the mechanism is one involving catalytic activity on the part of the boric oxide—for no oxide was found to be taken up by the low conductivity crystals—and this is confirmed by the fact that there is no reduction evident in the concentration of cation impurities in the low conductivity samples.

It seems that the boric oxide somehow induces the formation of a stable complex between the cation and anion impurities. This is lent further credence by the fact that when BDH 'Optran' grade NaCl—which is selected for its low anion impurity content—was used,

no low conductivity crystals could be obtained, presumably because there were insufficient anions present to form a complex with all the cation impurities and thus prevent them from creating vacancies which can participate in the conduction process.

The lack of extrinsic conductivity also allows the slope of the intrinsic line to be determined over a much greater range of conductivity than is usually possible. This in turn allows the accurate estimation of the enthalpy of formation of Schottky defects which are formed, for example, when atoms migrate to the surface of a crystal from its interior.



model for the nature of the packing, curiously reminiscent of TMV.

A very different, rather opportunist, approach to molecular shape in solution is one that Werner, Bunting and Cathou (*Proc. US Nat. Acad. Sci.*, **69**, 795; 1972) have applied to immunoglobulin. This is known to be a Y-shaped molecule, with the antigen binding sites at the ends of the prongs, and there is diverse evidence of flexibility about the hinge. Werner *et al.* have by recombination made a hybrid antibody, in which one site combines with the hapten, dansyllysine, the other with fluorescein. These form an excitation sensitizer-emitter pair. If the prongs of the Y are close together, excitation of the one will result in emission from the other by resonance transfer. The extent of transfer, which falls off as the sixth-power of the separation, will decrease rapidly with widening angle between the prongs.

Werner *et al.*, in fact, find no detectable transfer, which places a lower limit on the the separation of sites and thus on the angle, which turns out to be of the order of 90° . This implies that the Y tends to a T—a result that does not agree too well with the evidence of electron microscopy. Warner *et al.* insist that it is the latter which is misleading, rather than any of the factors involved in calculating the coefficient in the sixth-power relation.

NEURONAL SPECIFICITY

Ectopic Eyes

from our Neurophysiology Correspondent

In the normal development of the retino-tectal projection in *Xenopus laevis* (the notorious midwife toad), the antero-posterior and dorso-ventral axes of the eye are specified before the ganglion cell axons grow out to form the optic nerve. This was shown by Jacobson (*Develop. Biol.*, **17**, 202; 1968) who found that when an eye was taken from a stage 28 tadpole, rotated 180° and replaced in its orbit, normal connexions formed with the tectum. The same operation performed at stage 30 led to inversion of the retino-tectal projection in the antero-posterior axis; performed at stage 31 it led to inversion of both axes. Thus the axes are specified early, separately and only a few hours apart.

The specification of axes, or of polarities in both antero-posterior and dorso-ventral directions, presumably arises as a result of some activity controlled by information from a field set up in tissue surrounding the eye. It is commonly suggested, for example, that this information might reside in concentration gradients of specific chemicals, or morphogens, although, of course, any graded property could suffice. Positional information acquired from such

gradients is needed, at some time, by all developing tissues. Wolpert (*J. Theoret. Biol.*, **25**, 1; 1969) has suggested that such positional information may be specified by universal mechanisms, differences in final patterns of differentiation between different tissues or even between different species arising from different cellular responses to similar morphogenetic fields.

Hunt and Jacobson (*Proc. US Nat. Acad. Sci.*, **69**, 780; 1972) have tested this idea directly by transplanting early tadpole eyes from the orbit to the tadpole flank. If the idea were correct the eyes should be able to obtain positional information from the field supplying the flank, and should be able to use this information in the specification of their axes.

Four sets of experiments were performed. In the first, left eyes were taken from stage 28 tadpoles and were implanted in normal orientation in the right flanks of tadpoles at the same stage leading to inversion of the antero-posterior axes. As in all the experiments, the eyes developed normally. When they had reached a morphology characteristic of stage 32 they were transferred without inversion to the right enucleated orbits of final carrier tadpoles between stages 32–34.

The carriers were allowed to reach metamorphosis and were tested for responsiveness to a lure. The response could be either normal or misdirected. After metamorphosis the retino-tectal projection was mapped by recording the retinal position whose stimulation by a light spot produced the greatest neuronal activity at a given locus on the tectal surface. In this first experiment all behavioural and neural responses were normal, showing that the eyes had not “remembered” their antero-posterior inversion produced on transfer

from left orbit to right flank, but had specified their axes later, using information either from the flank or the final carrier orbit.

In the second series of experiments all procedures were the same except that right eyes were used so that there was no antero-posterior inversion, and a 10° inversion was made on transfer from the flanks to the final carrier orbits. In this case the carriers had misdirected responses and inversion of both retino-tectal axes. This showed that the eyes had in fact acquired their positional information from the flank fields, not from the final carrier orbits.

In the third group right eyes were taken from donors at stages 31–32, and were transplanted with 180° inversion to host right flanks and then with a further 180° inversion to the right orbits of stage 50 carriers. All carriers developed normal behaviour and connexions.

In the fourth group eyes were treated as in the third except that there was no inversion on the first transplant. Animals from these experiments had misdirected responses and inversion of both retino-tectal axes. These last two experiments show that eyes at stage 32 already have their axes specified and do not use positional information from the host flanks or the carrier orbits for re-specification.

All the left eyes of the final carriers showed normal connexions and visuo-motor junction. Hunt and Jacobson therefore conclude that positional information can be supplied from the flank field and that the eye can use it in exactly the same way as it uses information in the orbit. It is still not known whether this information supplies both the polarity of axes and the absolute position of the retinal ganglion cells, or whether the positions of the

Time Variation of Gravity Constant

ONE of the tests of the Brans-Dicke theory which is likely to produce conclusive results in the near future is the use of radar echo time delays over interplanetary distances. The test depends on the variation of G predicted in Brans-Dicke models — $|\dot{G}/G| \approx 10^{-11} \text{ yr}^{-1}$ for flat space and $|\dot{G}/G| \approx 3 \times 10^{-11} \text{ yr}^{-1}$ for closed space, both pressure-free. In next Monday's *Nature Physical Science* (May 29), R. E. Morganstern refines his earlier estimates of the time variation of G in pressure-filled flat space cosmologies (*Nature*, **232**, 109; 1971) to cover curved space models as well.

Morganstern finds that in the case of positively curved space the rate of change of G can be no more than an order of magnitude greater than the flat space limit; negatively curved

space poses a rather more difficult problem, and the limit on $|\dot{G}/G|$ depends critically on details of the cosmological models employed in that case.

But Morganstern's results are of interest because the best radar echo measurements available (Shapiro *et al.*, *Phys. Rev. Lett.*, **26**, 27; 1971) set an upper limit of $3 \times 10^{10} \text{ yr}^{-1}$ on $|\dot{G}/G|$. So the extreme upper bound on $|\dot{G}/G|$ in a closed, pressure-filled, positively curved Brans-Dicke cosmology is just greater than the observed limit.

It seems likely that as both theory and observation develop the range of permissible Brans-Dicke models left to cosmologists as possible representations of our universe will be rapidly narrowed down.

ganglion cells are only measured relative to a field within the retina as development proceeds.

The second alternative seems more likely, both because it is extremely unlikely that the retinal cells would have received, say, the correct absolute concentrations of morphogens in the flank field, and because Feldman and Gaze (*J. Embryol. Exper. Morph.*, **27**, 381; 1972) have recently shown that abnormal patterns of cell division within the developing *Xenopus* retina, caused by implanting two nasal half-retinas into one orbit, do not affect the specification of relative cell position within each half retina, which behave as a single field. Indeed, it is probable that this is continually respecified during development as the retina grows and its axons form connexions with the developing tectum. Any concentration or other gradients within a retina would have to increase in extent as the retina expanded and would presumably have the retinal margin as a reference zone.

ENTOMOLOGY

Resin Eaters

from our Soviet Correspondent

SOME insects live in what would seem to be most inhospitable habitats; the larvae of certain Diptera and Lepidoptera, for example, have found a niche in the resin of conifers, which they eat. Some of these resin eating larvae possess special adaptations for their mode of life; others do not. B. M. Mamaev, of the "A. V. Severov" Institute of Evolutionary Morphology and Animal Ecology of the Soviet Academy of Sciences, has found, for example (*Zh. Obshch. Biol.*, **32**, 501; 1972), that the resin feeding larvae of several Lepidoptera (for instance, *Evetria resinella* and *Laspeyresia zebeana*) are not protected in any special way against the liquid resin, other than by the silk cocoon which they spin as do other lepidopteran larvae.

On the other hand, Mamaev has found, from a study of conifers in the European, Central Asian and Caucasian regions of the Soviet Union, that some dipteran larvae are highly specialized for life in the conifer resin. These larvae have an integument which is impermeable to the toxic components of the resin, enlarged spiracles to maintain adequate respiration and an extra-intestinal digestive process for dealing with the substance.

Some of these resin eating dipterans, notably *Thomasiina*, are known to have an adverse effect on the development of young conifer seedlings so that further study on the adaptations and ecology of these resin eating larvae might be economically advantageous.

AEROSPACE RESEARCH

Effects of Altered Gravity

from a Correspondent

THE impressive advances made in space flight in the past decade have prompted much research, in the United States and in the Soviet Union, on the physiological and behavioural effects of altered gravity in space. Indeed, many workers in the field of aerospace medicine believe that the present limits of exploration in space are determined more by the tolerance of human astronauts suffering long periods of inaction and abnormal gravity than by the technological capabilities of the spacecrafts themselves.

There has thus been extensive research into the effects of altered gravity; Soviet cosmonauts, for example, have recently been testing as a counter-measure the effectiveness of exercise while they are in the Earth's orbit, and the National Aeronautics and Space Administration (NASA) has also sponsored research in the field of psychology which includes not only sensory deprivation, but also experiments on the effects of altered gravity on animals. Some of the latest research in this field has now been reported by D. F. McCoy and J. P. Jankovitch, of the University of Kentucky, who, in a NASA sponsored project, experimented with rats in a high gravity environment (*J. Comp. Physiol. Psychol.*, **78**, 305; 1972).

Previous work by, for example, Broderick and Lange, and Clark *et al.* (*Aerosp. Med.*, **40**, 747, 850; 1969) has shown that hypergravity (that is, gravity in excess of 1.0g) can act as an aversive stimulus and animals learn to perform tasks that will reduce this gravity. The experimental gravity is artificially produced by placing animals in parabolic centrifuges, with spiral tracks along which they are free to move. According to their radial distance from the axis of rotation, therefore, the animals can adjust the magnitude of their resultant centrifugal acceleration, and thus the value of the artificial gravity they prefer best.

One problem arising from this evidence is that it is not clear whether these animals (usually rats), reared in normal gravity, were seeking the lowest gravity available in the apparatus, or were merely selecting the environment most similar to the one in which they had become habituated. An apparatus to control this effect by providing hypogravity (that is, gravity from zero to 1.0g) as a choice would, however, require orbital flights.

McCoy and Jankovitch had a more practical approach to the problem by rearing rats for long periods (up to one year) in a high gravity environment (2.0g) and then giving them preference tests over a gravity range of 1.0 to 2.0g. During the testing, care was taken to bank the locomotory track to ensure that the resultant gravity vector was

Basis of Stringent/Relaxed Control

THE so-called stringent response remains one of the more enigmatic regulatory systems which control gene expression in *Escherichia coli*. When wild type *E. coli* are starved of amino-acids a variety of seemingly unrelated biochemical pathways are shut down. There is good evidence to believe that the gene product of the RC^+ gene, which confers this stringency, mediates its effect by catalysing the production of an effector molecule, magic spot or ppGpp from GTP. The RC^+ gene product must therefore presumably be able to recognize GTP and there is also good evidence that the RC^+ gene product can also recognize all species of aminoacyl-tRNA.

When Cashel and Gallant first reported the discovery of ppGpp and its involvement in stringency they noted this dual specificity of the RC^+ gene product. They also suggested that the RC^+ gene product might prove to be a factor involved in the elongation of nascent polypeptide chains, for such translation factors have the required dual specificity. Hall and Gallant, as they report in next Wednesday's *Nature New Biology* (May 31), have pur-

sued this idea and have come up with further circumstantial evidence consonant with it.

If the RC gene specifies an elongation factor, which, when there are plenty of amino-acids, catalyses chain elongation but which "idles" in the absence of amino-acids to produce ppGpp, RC^- mutants that have lost the ability to produce ppGpp and the stringent response should contain a mutated chain elongation factor. This factor because of the mutation might fail to function properly during protein synthesis; it might, for example, cause enhanced misreading in $RC^- E. coli$.

In particular, Hall and Gallant compared the β galactosidase made by RC^+ and RC^- cells during starvation for arginine and found that the RC^- enzyme has a lower specific activity and a greater sensitivity to heat denaturation than has the RC^+ enzyme.

Other than by postulating changes in the primary structure of the subunits of the RC^- enzyme it is extremely difficult to account for these observations which, of course, are those predicted by the hypothesis that the RC gene may specify an elongation factor.

transmitted dorso-ventrally (as for a quadruped in a normal environment), and the rats were constantly monitored with a television camera.

The results show that there was a significant preference for high gravity for animals reared in high gravity, although this proved to be transitory insofar as 60 per cent of them later exhibited a preference for lower gravity in subsequent tests. Nevertheless, there are grounds for believing that hypergravity is not necessarily aversive, and would depend to a great extent upon the g level to which the animals were habituated. This agrees with evidence of other aversive stimuli; for example, prolonged exposure to light, where Walters (*Canad. J. Psychol.*, **17**, 412; 1963) has shown that aversive stimuli can lose their negative properties as a result of prolonged exposure.

McCoy and Jankovitch are cautious of making premature speculations, but point out that these results are in accord with more general theories of adaptation levels (for example, Helson, 1964), and can be interpreted as the "neutralizing" of aversive factors by repeated stimulation. If this is the case, then by analogy a similar effect may still be shown in the case of hypogravity, and be of great use in the habituation of prospective astronauts for long periods in conditions of zero gravity.

OPTICS

Diffraction Gratings

from a Correspondent

A CONFERENCE on diffraction gratings was jointly organized by the Spectroscopy and Optical groups of the Institute of Physics, at Imperial College, London, on April 21, and it is a measure of the interest in this topic that a meeting originally planned to cater for local interests attracted thirty European participants and representatives from most of the principal US manufacturers. The conference was supported by an exhibition of products.

The opening session on classical, that is, ruled, diffraction gratings began with a review by Dr E. Loewen (Bausch and Lomb) who surveyed the art of ruling, and illustrated how recent developments have led to improvements in ruling engines, especially their interferometric control and the building of even larger ones. The purpose is to provide gratings for raman spectroscopy, where the need is for fine pitch combined with minimum ghost intensity and for large gratings for astronomical spectroscopy (this last topic was amplified later by Dr R. C. M. Learner (Imperial College, London) who described the plans for the establishment of a large grating ruling laboratory at the Tucson head-

quarters of Kitt Peak National Observatory). It became clear that concave gratings have assumed greater importance since space astronomy opened up the XUV wavelength region, which no other type of dispersing element can function effectively. Here the chief problems are the generations of grooves that are smooth even in terms of wavelengths one tenth that of visible light and the supply of novel special purpose rulings, for example, multi-partites with varying groove spacing and aspherics.

Dr R. Jarrell (Jarrell-Ash) and Dr H. W. Faust (Carl-Zeiss, Oberkochen) completed the session on classical gratings by reviewing their work on the intensity distributions for blazed gratings (J-A) and the development of a new 6 inch ruling facility at Carl-Zeiss.

The highlight of the meeting centred on developments in the field of interference gratings, in which progress is very rapid. In this technique the fringe system produced by two interfering coherent beams is recorded on photoresist and it is then translated by various processes into the corresponding metallized grating. In contrast to ruled gratings, however, the grating frequency and size are not limited by the problems of diamond wear and optimum width. Furthermore, the fringes, being contours of constant optical path difference, may be created and recorded to give predetermined imaging qualities to the final grating. Dr J. Flamand (Jobin-Yvon) reviewed the general conditions for stigmatism for such gratings and showed how holographic solutions have already been achieved for several applications in astronomy and spectroscopy.

In further practical vein, Dr M. C. Hutley (National Physical Laboratory) discussed the progress of his laboratory in obtaining blazed profiles and high

photometric efficiencies over significant regions of the spectrum. The Littrow mounting was shown to be the most favourable configuration to use to avoid the polarization anomalies that arise from the characteristic sinusoidal profile of interference gratings. The properties of these anomalies drew the attention of several speakers but it was apparent that more data were needed, particularly in the vacuum ultraviolet region. Mr A. Termanis (Rank Precision Industries Ltd) showed the first data of this type in the wavelength region 1670 Å-400 Å. The anomalies are observed as rapid variations in response over short wavelength intervals, but in most cases it seems that the presence of these features would not preclude the use of interference gratings in spectroscopic instrumentation.

The final session dealt with the subject of gratings for X-ray use. Dr A. Franks and colleagues (National Physical Laboratory) showed how development of the NPL phase grating of rectangular groove profile and with optically smooth diffracting surfaces has led to the short-wave performance being extended into the hard X-ray domain. It is now a practical proposition to consider the construction of a focusing X-ray grating spectrograph for the wavelength region 0.1 Å-5 Å (2.5 keV-125 keV).

The session concluded with a summary by Dr R. J. Speer (Imperial College, London) of the new facilities that are available for the standardization and optimization of gratings in the X-ray and soft X-ray regions of the spectrum. This research is in collaboration with the major manufacturing companies and work is well advanced on a reference collection of data for all rulings that are currently available as replicas.

Predicting Structural Distortions

IN next Wednesday's *Nature New Biology* (May 31) Low and Spitz report the results of their attempts to predict whether an amino-acid substitution in human adult haemoglobin will or will not result in a disturbance of the structure (and therefore perhaps to physiological consequences).

The approach is very simple: all amino-acids have been characterized by various authors in terms of helix-promoting potential. Low and Spitz in fact characterize triplet sequences as helix-forming or non-helix-forming. If the central residue of the triplet is replaced, the helix-forming tendency may either change or remain the same. Low and Spitz have examined the substitutions in the 104 human adult haemoglobin variants of known sequence, which they classify into those that

should be unaffected by the substitution, those in which a helix-breaking substitution occurs in a helical region and those with a helix-promoting substitution in a non-helical region.

Fifty-eight substitutions occur in wholly helical regions and thirty-one of these are predicted to cause distortions, whereas among the twenty-two in clearly non-helical regions, eleven cause predicted distortions. In one case a proline is introduced into a long helix, and must without question cause some disruption.

So far only four such predictions can be verified in terms of X-ray structures: in one case a predicted distortion occurs, in another no distortion is expected or observed, and in two further cases disturbance of non-helical regions is predicted but not observed.

SURVEY OF SCIENCE IN EUROPE

Prospects for European Collaboration

THE pages which follow show that in the past year there has been still further progress towards sensible machinery for international collaboration among European scientists and technical people and that, for better or worse, the European Commission in Brussels is beginning to play a constructive part in that process. The past year, and those preceding it, have also produced some notable successes. CERN at Geneva is a model for everyone, and soon there will be EMBO to follow. Laboratories all over Europe have become foci for collaboration, especially when research depends on the accessibility of large and expensive instruments in high energy physics and astronomy, for example. There have also been some successes in technological development.

Against all the odds and in spite of the way in which Euratom became moribund half way through, the project for developing a high temperature reactor has turned out well, and may yet make a useful contribution to nuclear technology. The ambitions of the European Commission to sponsor technical collaboration in the development of computers and telecommunications have fared less well, in spite of the energy of M. Pierre Aigrain. European space research continues to flounder, possibly because Europe is not ready or has no need of a gigantic organization like NASA in the United States, while there remains a danger that European governments will be seduced into spending money on rockets for launching satellites before it is apparent that the money would be usefully spent.

So far, most of the institutions for scientific and technical development in Europe have been created in response to particular needs and have come into being only when the case for their existence has been strong enough to overwhelm the objections to them, logistical and chauvinistic. In the circumstances, the attempts so far at creating forms of international collaboration on a European basis have been patchy, to say the best of them. And it remains a scandal that in several important technical matters, the development of fast nuclear reactors for example, nothing of substance has been done to prevent four governments—the British, French, German and Italian—from spending money on the separate development of reactors which are, in their essentials, the same. Worse still, European governments and private companies are still hard and separately at work on the development of the engineering equipment on which modern telecommunications are based (which is not of course to say that consumer products such as television sets cannot be transplanted from one country to another).

It is true that the British and French governments have collaborated on the development of a supersonic aircraft, but that is almost the exception that proves the rule—commercial organizations, however large, would not have sunk good money into such an uneconomic enterprise, and the chances are that each government would have been saved from its folly at an early stage if it had not been bolstered up by its partner. In short, in spite

of all the tangible benefits which the European Communities have already brought in economic fields, European industry and the scientific research on which it depends are still far from tightly coordinated.

What is to be done? The most immediate need in Europe is that the dependence of advanced industries on the national markets should be lessened. In Britain, for example, the Post Office Corporation is still largely dependent on British made equipment; and it will be many years before it switches over, of its own accord, to manufacturers from the mainland. Defence equipment is again largely a domestic purchase for European governments, even though there have been some sensible arrangements for the collaborative development of new aircraft and tanks. In the long run, however, it is clear that this chauvinism in the purchasing of advanced equipment will restrict enormously the efficiency of European industry in a properly European context.

In the circumstances, what is needed is an understanding among European governments that public purchasing of advanced equipment will in future be determined not by the geographic origin of the products but by considerations of price, pure and simple. The European Commission could do an important public service if it spent less energy huffing and puffing about particular schemes for collaboration and more on urging member governments to adopt such policies. In the same breath, it could usefully take steps to urge that European governments should begin to think how best to pool their collective resources in the conduct of basic research through universities and other institutions. For several years Europeans have been talking in a desultory fashion of institutions that would distribute funds for basic research to all comers. In present circumstances that is an unattainable goal. But is it not high time that existing grant-giving bodies gave money to university research groups outside the national frontiers within which they are at present confined? And should not the research councils take foreign nationals onto their committees? In the long run, one of the biggest objections to Lord Rothschild's recipe for the organization of basic research in Britain is that it says nothing of the rest of Europe.

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EUROPEAN ECONOMIC COMMUNITY

Science, Technology and Industry in the EEC

by our Special Correspondent

Although several cooperative ventures in science and technology have been developed in Europe since the Second World War, the European Economic Community must decide on a European policy if the most is to be made of future possibilities.

EUROPEAN cooperation in science and technology in the past twenty-five years has consisted of a handful of projects which, in the policy sense, have been almost completely *ad hoc*. This is not to say that, from a scientific point of view, an institution like CERN has not been a remarkable success, but a framework within which the need for such bodies has been determined is almost non-existent. And the inevitable feelings of national chauvinism have contributed in no small way to the rather teetering forward steps of bodies like the European Space Research Organization (ESRO) and the European Launcher Development Organization (ELDO), to say nothing of some of the research activities that are now carried out under the Euratom Treaty on a basis that can be best described as hand to mouth.

The six members, soon to be ten, of the three European communities—the coal and steel community, the atomic energy community (Euratom) and the economic community (EEC)—have been intimately involved in many of these large ventures, yet in the past they have spoken with six voices instead of one. Why should this be and what moves are being made by the commission of the communities, which is situated in Brussels (Fig. 1) and which is responsible for drafting policy and implementing the directions of the decision making Council of Ministers, to rectify the situation?

Restrictions

The key to understanding why progress towards cooperation in science and technology and towards a coherent research and development policy within the EEC seems at times to be so excruciatingly slow lies in the treaties that set up the three communities. Although the Euratom treaty provides for nuclear research and development and the coal and steel treaty for research of particular relevance to coal and steel production (the health of miners, for example), the EEC treaty makes no mention of scientific and technological matters. Nor for that matter does it mention the industrial reorganizations and regrouping that may be necessary to take best advantage of research cooperation in such research intensive fields as computers and aerospace. The commission is thus, strictly speaking, powerless to intervene in such matters, although it can of course try to persuade the Council of Ministers to extend its powers either by implementing the article in the EEC treaty which permits the council to give the commission a specific new task or by using the open-ended nature of the treaty to add extra protocols which would include references to scientific matters and to industrial politics in its widest sense.

In spite of this legal impasse, the Council of Ministers of the Six did set up in 1964 its Medium Term Economic Policy Committee, which itself quickly spawned a working group on scientific and technical research policy—now known as the Aigrain group after its chairman Professor Pierre Aigrain.

Both committee and working group are, of course, strictly speaking independent of the commission, although there is a great deal of cooperation between the two. The trouble is that the Aigrain group has essentially political origins and is made up of delegated national representatives, so its history reflects quite accurately the political atmosphere within the EEC during the 1960s. In 1968, for example, the Netherlands adopted blocking tactics as a reprisal for the refusal of France to countenance the enlargement of the community, and it was December of that year before the group could resume its work. Finally, however, in October 1969, the Council of Ministers accepted the report of the working group, which had shortlisted projects in seven chief areas from the seventy-one projects it originally envisaged.

Schemes for Cooperation

Although the possible areas of collaboration—in data processing, telecommunications, pollution and noise, meteorology and oceanography, new means of transport, and metallurgy—were singled out by the Aigrain group—a committee set up by the EEC countries alone—the council decided to invite nine other European countries, namely Denmark, Norway, Britain, Ireland, Austria, Portugal, Spain, Sweden and Switzerland, to become involved in a programme of projects which the Council of Ministers was the first to admit was modest, but which it felt could form the basis of more extensive and coherent cooperation. As time went on the number of countries involved or interested increased to nineteen by the addition of Greece, Yugoslavia, Finland and Turkey.

By the end of 1970, the nineteen European nations (represented by senior officials on a committee which came to be known as the COST group—*coopération européenne dans le domaine de la recherche scientifique et technique*) had agreed in principle to cooperate in the seven areas suggested by the Aigrain group (also known as the PREST group), and seven working parties were duly set up to draw up specific agreements to regulate the cooperation in each field.

One of the early casualties in the COST group's working party on data processing was the proposal to develop a large European computer by 1980, with a view to competing with the large American companies. It is generally reckoned that the project would have cost at least \$500 million a year. In the event the European computer industry fought shy of the idea because it considered that the market for such a machine could not be predicted with sufficient accuracy and that the risks involved were generally too great. Both companies and governments said that the other should pay the lion's share, and the project was quietly shelved.

On the positive side, however, some \$5.3 million is now earmarked by eight countries, together with Euratom, for the development of a European data transmission network linking centres in France, Italy, Switzerland, Britain and one of the laboratories of the Joint Research Centre of Euratom. The original proposal to create a European Institute for Data Processing and Technology—to provide appropriate training on a European basis—has been placed in cold storage, but not abandoned, because separate discussions on the subject are being carried on between some of the members of the EEC themselves. The setting up of a European computer program library, at a cost estimated to be about \$3.5 million, is also being considered by the data processing working party, but the outcome will probably be a compromise between the view

of most of the participant countries that it should be located at the Euratom laboratory at Ispra (Fig. 2) and the British view that it should be at the National Computing Centre in Manchester.

Research and planning in the field of telecommunications seem to provide a good example of a project which is being implemented almost in full, as suggested by the PREST group. The working party on telecommunications set up by the COST group is, for example, already considering a special study on the needs for telecommunications services during the next year or two, and a more general study of telecommunications needs in 1985 is expected before the end of this year. On the experimental side, however, only France, Italy, Yugoslavia and Switzerland are engaging in a project costing about \$120 thousand that is concerned with the development of microwave aerials. A second project costing about \$220 thousand is still awaiting the final decisions of the governments that have expressed interest.

Another subject picked out by the PREST group as ripe for development on a European basis was metallurgy, particularly work on materials for gas turbines and for desalination plants; the former has been taken up in an agreement between eight states and Euratom which provides for the expenditure of about \$7.7 million from both public and private sources during the period up to the end of 1974. The latter agreement involves only six countries—Germany, Spain, France, Italy, Yugoslavia and Austria—and about \$2.2 million.

Although pollution research accounts for only about one third of the expenditure of some \$22 million (Table 1) which has so far been agreed on by the COST group, the progress of the various cooperative ventures which have been agreed during the past year will be watched with particular interest because of the statements of policy on environmental matters which have been made by the Commission of the European Communities; these are to the effect that relevant laws within the EEC countries should be harmonized, as should methods of management. It is perhaps not surprising to find as many as eleven countries agreeing to coordinate parts of their national research efforts in air pollution and water pollution. What many will find disappointing is that the agreements are not bolder and more far reaching. The \$2.1 million to be spent during the next four years on air pollution, for example, is confined to studies of the ways in which sulphur dioxide is absorbed by plants and soil and to the perfection of measuring methods and the exchange of data. A project on desulphurization of fuels and flue gases had to be dropped because of a lack of the necessary experimental facilities, in spite of considerable interest being expressed by some ten countries. The pollution working party will,

Table 1 Costs of Some of the Agreed Cooperative Programmes Arising from the PREST (Aigrain) Proposals

	Cost \$ million	No. of countries participating	Duration (years)
Data processing	5.3	8 *	5
Microwave propagation	0.120	4	2
Metallurgy	9.9	10 *	3
Pollution:			
Air	2.1	9 *	4
Water	4.0	11 *	3
Sewage sludge processing	1.1	9	2
	22.5		

* There is a contribution from one or other of the European communities. Total from this source is \$2.2 million.

however, probably resurrect this project at a later date, when it may be possible for the COST group to recommend a project of this kind.

Much of the coordinated work on water pollution during the next three years will be directed towards improving the methods of analysing very small amounts of organic pollutants in water rather than performing any particular measuring or monitoring activity. The list of analytical tools to be developed and improved includes chromatography (vapour phase and liquid-liquid chromatography, for example) and the use of chromatography and mass spectrometry in combination.

It is, however, important to realize that a substantial number of these European cooperative ventures do not involve new money, but the redirecting of money already allocated nationally in such a way that the work involved makes economic sense from a European point of view.

Future Projects

Several other projects, apart from the computer program library mentioned previously, are also being actively considered. Undoubtedly the largest of these is the proposed European medium-range weather forecasting centre, which, it is planned, should use a very large computer to provide forecasts four to ten days ahead. As well as this it should be a centre for postgraduate training and for the provision of services to national meteorological establishments. The meteorological working party of COST is looking into the way in which such a centre could contribute to international endeavours such as the World Weather Watch and the Global Atmospheric Research Programme (GARP). Present estimates of the cost of such a venture suggest that about \$22 million will be required during the first five years and about \$8.2 million a year afterwards. One of the problems that has yet to be resolved is the location of the centre. The Commission of the European Communities is in favour of one of the Euratom laboratories, but other locations like Frankfurt and the British Meteorological Office at Bracknell have also been put forward. In this case, however, it seems unlikely that a compromise of dividing the centre between two locations, as may be the outcome of the discussions on the siting of the computer program library, will be considered at all suitable. The meteorological centre would employ about 120 people and is likely to be supported by all the countries in the COST group with the possible exceptions of Greece, Turkey and Finland.

New means of transport were also mentioned by the PREST group in its report to the Council of Ministers two or three years ago, and there is now agreement in principle to a study to be undertaken by the Organization for Economic Cooperation and Development of inter-urban passenger transport in the future.

Among ideas that are still a long way away from translation into reality are a programme of work on electronic

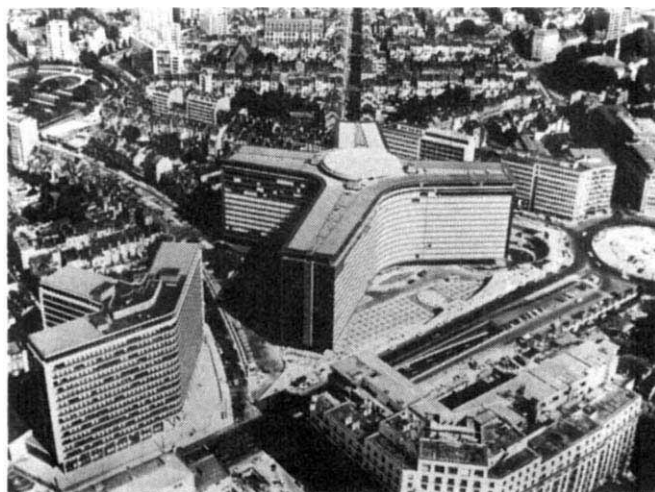


Fig. 1 The headquarters of the Commission of the European Communities in Brussels.

Table 2 Expenditure on Medical Research in Some European Countries and the United States (1967)

Country	Expenditure \$ million
Belgium	20.4
France	138.7
Germany	156.4
Italy	51.2
Netherlands	56.9
EEC total	423.6
Britain	137.1
United States	1,444.1

traffic aids on roads, the study of the feasibility of building a hovercraft weighing about 2,000 tons (the present limit being about 300 tons), and cooperation in research on superconductivity. A further project that could tie in well with the meteorological centre is the proposed network of buoys around the European coastline which would measure parameters of meteorological and oceanographical interest. At present the details of the costing remain to be worked out, as does the design of the buoys and of the equipment they will carry.

The PREST group has by no means been inactive during the past two or three years, during which its original proposals to the Council of Ministers have been slowly but surely progressing through the complex of committees and working parties to eventual international agreement. In particular it has been directing itself to the problems of the interchange of scientific information, to a comparison of the national research programmes of the members and prospective members of the EEC, and to singling out areas of medical research which might lend themselves to European cooperation. In order to discharge the last of these tasks, it has set up a *groupe de conception*, comprised of practising scientists rather than senior government officials, under the chairmanship of Professor G. Henneberg, president of the Bundesgesundheitsamtes (German Federal Health Bureau), Berlin.

This group first met in July 1971 and has now produced a report suggesting three areas of research in which cooperation should be encouraged, namely epidemiology, medical biology and biomedical engineering. It also suggests that such topics as the contribution of environmental and genetic factors in respiratory diseases, the psychological, physiological and metabolic aspects of ageing, and the causes of road accidents would go to make up particularly timely joint projects. The *groupe de conception* also advocates the setting up of three *ad hoc* groups to establish precisely where co-operation in the three chief fields would be most valuable and to encourage the exchange of research personnel at all levels and for both short and long periods of time. The scale of medical research in Europe is shown in Table 2.

Community Research

Although the member countries of the European communities may, as individual European states, associate themselves with the ventures initiated by the PREST group, the commission of the communities has to be more selective about the research projects which it thinks of setting up or supporting. To be sure, Table 1 shows that through the Euratom or coal and steel treaties, the commission can engage in joint ventures, but it would not be able to initiate its own research, or even finance research elsewhere, on, say, the environment—except insofar as that kind of research is covered by these two treaties or is specially provided for by the Council of Ministers.

Most of the commission's budget for research in the nuclear field is spent at the Euratom Joint Research Centre—actually four laboratories, at Ispra in Northern Italy (Fig. 2),

Petten in the Netherlands, Geel in Belgium, and Karlsruhe in Germany. The history of these institutions has been by no means happy, for since 1968 the budget has only been approved from year to year rather than for several years at a time as was the case during their first ten years of existence. The Council of Ministers did, however, approve a scheme in December 1970 which would decentralize the planning and day to day running of the centre's research programme and allow it to expand its activities outside the nuclear field by accepting contract work from outside organizations.

But when the commission put forward a scheme for a three-year programme to start at the beginning of this year, there was disagreement at the Council of Ministers which led to the adoption of yet another stop-gap programme for one year only. The commission's proposals for 1972–74 would have cost \$256 million and would have included, for the first time, \$38 million for a limited programme of non-nuclear activity; for example, pollution research and the development of metallurgical standards. As it is, the budget for 1972 stands at \$44 million of which only \$4 million is for non-nuclear research.

The wrangles that have taken place over the 1972 budget for the Joint Research Centre do not augur well for its future. In particular there is a trend towards the increasing fragmentation of what should ideally be one common programme financed by all the members of the EEC together with a complementary programme supported only by some. Of the 1972 budget of about \$44 million, for example, no less than \$28 million is earmarked for the complementary programme and only \$17 million for the common programme. The situation was worse at the end of last year when the proposal for a three-year research programme was being considered, for it was apparent that in those circumstances France would force a very substantial proportion of the projects into the complementary category, including all the non-nuclear activities that were to appear for the first time. The other members of the EEC thus felt that rather than accept a diluted three-year programme, they would sanction a one-year programme and await the enlargement of the community in 1973 before committing themselves to more long term plans.

The community's own research is, however, not all at sixes and sevens. About \$25 million is, for example, to be spent by the commission between 1971 and 1975 on its biology programme, and about \$10 million a year is earmarked for the fusion programme. The biology programme is, of course, concerned with nuclear aspects of the subject, and the study of the effects of radiation on living matter forms a predominant part. By contrast with the 1972 budget for the Joint Research Centre, some \$19 million of the total funds



Fig. 2 The laboratory of the Euratom Joint Research Centre at Ispra, Italy.

allotted to the biology programme are supported by all six members of the EEC, and of this about \$5 million is spent at Ispra, though not by the Joint Research Centre as such, and about \$12.7 million in the form of contracts with universities and other institutions in the member countries. The remainder is devoted to the funding of the headquarters of the programme in Brussels. The complementary programme, supported by the Netherlands, Germany and Italy, is concerned with the applications of nuclear techniques to agricultural and medical research, and will entail the expenditure of about \$6.2 million by 1975. One of the contracts in the biology programme, for example, is shared between the Universities of Ulm, Milan and Brussels, the Dutch TNO laboratory at Rijswijk, and the Cancer Institute at Villejuif, France.

The programme of fusion research is carried on through contracts with such bodies as the laboratories of the French CEA at Fontenay-aux-Roses and Grenoble, the laboratory of the Italian CNEN at Frascati, and the Institute of Plasma Physics at Garching, Germany.

Future Policy

Clearly the research programmes sponsored or encouraged by the Commission of the European Communities have in the past been deficient to some extent, but they have also recorded some successes. Thus although Euratom has patently failed to generate a coherent European nuclear industry, the successful implementation of the commission's biology and fusion programmes has shown that community financed research can be viable, provided it is done on a contractual basis in the centres that already exist in the member states of the community. And the agreements that have been forged between European nations, not necessarily in the EEC, as a result of initiatives emanating essentially from the Council of Ministers of the EEC, have shown that the basic will is there even if the action may at times seem to be passive and slow. But what is the next step to be taken within the EEC itself to ensure that future cooperation in science and technology in Europe follows a coherent policy which can be implemented without the political overtones that have accompanied most European ventures in the past?

These problems are being faced at the headquarters of the EEC commission in Brussels by the department responsible for industrial, technological and scientific affairs, which is headed by Mr Altiero Spinelli, one of the nine members of the commission; his responsibilities also include the Joint Research Centre, independent though this now is in many ways, administration of the customs union, and training and education. Mr Spinelli has as his special adviser an Englishman, Mr Christopher Layton, who was until the end of last year Director of the Centre for European Industrial Studies at the University of Bath.

The directorate-general for industrial, technological and scientific affairs is responsible to Mr Spinelli, and is one of twenty concerned with all aspects of the activities of the EEC; each is within the field of competence of one of the members of the commission. Of the 5,400 people who work at the commission's headquarters, more than 300 are involved in Mr Spinelli's single directorate-general, which itself is divided into seven directorates responsible for groups of industries, or for matters like industrial and technological policy or for scientific policy and coordination of research.

What sort of thinking is going on among this body of people? Certainly there is a feeling that the enlargement of the EEC at the beginning of next year will give it something of a "critical mass" as far as communal research and development is concerned. And it seems that there is an increasing realization among individual nations that they cannot afford to spend the money required to develop "big" technology, and that the efficiency with which this kind of work is done must be improved; this, it is felt, will lead to a willing-

ness to establish institutions that will look to the commission for their funds rather than to the individual nations or to the Council of Ministers. The trouble is, of course, that the political implications of such a move are enormous.

New Institutions

Assuming no reluctance on the part of the nations of the EEC to give the commission a fairly free hand in working out and financing the consequences of what is essentially a European industrial policy, what new institutions would be required?

First, the commission has it in mind to recommend the setting up of a European Committee on Research and Development (CERD) which would be headed by a chief scientific adviser to the commission and staffed by some ten to twenty experts from industry, universities and government institutions of the member states. CERD would be responsible for establishing the priorities in European research and development and this would involve not only examining the European effort in, say, environmental research and recommending how the commission should support it financially, but also making recommendations about the way in which the research and development implicit in some European policy for industries such as the aerospace industry or the telecommunications industry should proceed.

Second, the commission is considering the possibility of creating a European Science Foundation which would concern itself with basic research at a European level. This, it is felt, should be a body which will not only organize and encourage European seminars and workshops in different fields, but encourage the mobility of scientists both within Europe and between Europe and the rest of the world. And, perhaps most important of all, it should be able to stimulate European centres of excellence by the distribution of a certain amount of money, and should develop into that body which will assess and initiate large new projects in basic science—be it a radiotelescope, a high flux reactor or another body along the lines of CERN.

Whatever else CERD may do if it comes into existence, it is unlikely to encourage the idea of the EEC carrying out the greater part of its common research and development programme within community research centres. The much stronger possibility is that the work initiated on the advice of CERD will be in the form of contracts involving funds from the commission, funds from member countries, and funds from interested industrial concerns. This concept is already operative in the EEC's fusion programme, for example, for the money provided by the commission is generally only 20 to 30 per cent of the total cost.

The important feature of any viable European policy for research and development is that there should be implicit in it some sense of evolution. Thus it might be thought desirable to earmark a fixed proportion of the EEC's budget for research and development and for the furtherance of industrial policy in general. The community's budget for 1972 is some \$4,000 million, and an entirely plausible suggestion would be that 3 per cent—\$120 million—should be devoted to industry, science and technology (including the Joint Research Centre). Indeed, there are those who speculate that ultimately a reasonable way of distributing the community budget would be to share it equally between agriculture, industry, science and technology, and regional planning.

A European Economic Community with a well defined policy for science and technology would, of course, be aware of the continuing importance of cooperation with other European nations, so it seems that there would be an important role to be played by COST, the committee representing nineteen European nations. There is certainly no reason why an institution like CERD or the European Science Foundation should not work closely with non-member states, and COST could provide just the link required.

MAX-PLANCK-GESELLSCHAFT

A Model for "Small Science"?

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This year the Max-Planck-Gesellschaft, Federal Germany's unique arrangement for the funding of independent research, enters on an unprecedented programme of structural reforms. Its sixty-year evolution may well provide object lessons for other European governments.

THIS year the Max-Planck-Gesellschaft (MPG), Federal Germany's chief instrument for independent research, enters the seventh decade of its existence with a programme of reforms which, if carried through, seem likely to remove the last vestiges of the authoritarian tradition which has haunted it since its beginnings in the Kaiser Wilhelm era and which has recently led to increasing disaffection among the younger scientists in its institutes. This undoubted turning point is also marked by a change in the presidency, Professor Reimar Lüst taking over from Adolf Butenandt who, in his term of office of eleven years, can be said to have completed the period of consolidation following the post-war revival under Otto Hahn.

Although their full effects may not be seen for some time, the present reforms in the MPG and the structural stresses which have led to them will be of considerable interest to all who are concerned with science policy on a large scale and the cost-effectiveness of government sponsored research. Nevertheless, the task of evaluating them, and indeed the Max-Planck organization as a whole, is an extraordinarily difficult one, set, as it must be, against an intricate historical background and a financial and administrative system quite unlike that in any other country prosperous enough to afford the luxury of such a large, self-governing research enterprise.

What comparisons there are seem tenuous; the MPG has certain affinities with the French Centre National de la Recherche Scientifique (CNRS) and with the Weizmann Institute in Israel, but has very little at all in common with the fragmentary system of government research stations in Britain, with the all-powerful academy institutes in Eastern Europe or with the assorted contract research and foundation laboratories in the United States.

The Max-Planck organization, like its forerunner the Kaiser Wilhelm Gesellschaft (KWG), is different from all these and quite self-consciously so. Although it receives all but a minute proportion of its income from outside interests—state, industry, and foundations—it remains a legally autonomous body with as much independence of action as is enjoyed by British universities, yet without any obligation to instruct or train in the academic sense. The roots of this freedom go far back to the liberal humanism of the early nineteenth century, for the 1911 foundation charter of the Kaiser Wilhelm Gesellschaft was a deliberate expression of the need, first noted as early as 1810 in the writings of Wilhelm von Humboldt, for supporting institutes (Hilfsinstitute) which would occupy a balancing position between the Academy of Science on the one hand and the universities on the other. Later the growth of industrial research and the large foundations added a fourth dimension to this scheme,

creating the necessity, as the first president of the KWG, Adolf von Harnack, was to put it, "... to lead capital into a clean bed under aegis of state and academy".

It was the civil servant von Harnack who, almost a hundred years later and with the support of Nernst, Emil Fischer and others, seized the psychological moment to restate Humboldt's case for separate institutes in its modern setting and with the additional urgency which the increasing industrial penetration of research had given it. The group which drafted the 1910 Denkschrift for the Kaiser did so with considerable tact, some side glances at the Royal Institution and the Lister and Carnegie Institutes, and an undisguised appeal to the nationalism of the day. It is doubtful whether their proposals could have taken root quite so quickly anywhere else than in the peculiar soil of kaiserly patronage, industrial pride and relatively benign capitalism which was then at its richest and about to be destroyed. In the event the foundation proclamation followed within months, the first institutes opened in Berlin and the society quickly grew strong enough to survive the disintegration following the First World War. In fact it not only survived but went on to weather the economic crisis of 1929, the further destruction of 1939–45 and to rise again under the new colours of the Max-Planck-Gesellschaft; it had, from the very beginning, shown an almost steady increase in both the number and the diversity of its institutes.

The story of how the KWG maintained its integrity in the face of pressures from government, industry and, for a brief period, the occupying powers is fascinating and, at times, dramatic. Von Harnack himself, nearly eighty by the time of the 1929 depression, fought successfully against a determined take-over bid by the Prussian Ministry of Culture; there were difficulties for some of the institutes under the Third Reich (though it seems that there was nothing like the loss of scientists suffered by the universities) and in the very ruins of Berlin a small band of enthusiasts—Butenandt, Heisenberg, von Weizsäcker, Hahn and others—conspired successfully for the eventual re-emergence of the society as a mainstay of a post-war revival of German science.

Growth over Ten Years

Today the Max-Planck-Gesellschaft comprises some fifty-two institutes and lesser research units, employs some 8,000 people, of whom some 2,000 are graduates, and operates on an annual budget of some DM 500 million (£60 million). This represents a quite phenomenal increase in scale over the past ten years—the corresponding figures for 1961 were forty-two institutes, 800 graduates and DM 80 million. (Because of closures and rearrangements, however, the actual number of institutes has remained at fifty-two since 1964.) Apart from some special income for space science and nuclear activities, a now relatively diminished contribution from industry and the foundations, and a very small profit of its own, the annual budget has, until recently, been raised in deliberately equal proportions from the federal and regional governments. There are signs, however, that this system of equal division of costs is breaking down; the huge Plasma Physics Institute at Munich has been funded with only a token 10% from the Bavarian authorities and there are said to be difficulties with other regions where

it has been known for local politicians to use the issue of Max-Planck contributions to put quite irrelevant pressure on the central government. The federal contribution is in part direct and in part funded through the Ministry of Science and Education. Some of the grant is earmarked as, for example, in the case of the Plasma Physics Institute, but this is exceptional. Although some institutes have specially close ties with industry, for example, the one for iron and steel research, there is no contract research as such and nothing that, with the worst will in the world, could be said to be of direct military importance. (An institute which accepted money from the US Air Force some years ago was very quickly reprimanded.) There is, in fact, a specific prohibition of all secret work, and freedom of research, which is incidentally guaranteed under the Federal German Constitution, is taken very seriously indeed. The relevant article reads, with poignant simplicity: "Art, science and scholarship are free. Truth is binding notwithstanding academic freedom."

The government of the MPG takes place through a number of bodies in which political, scientific and economic interests are balanced. The principal policy-making body is the senate with up to thirty-two elected senators, together with federal and regional government ministers, honorary figures and the three section presidents of the scientific board (Wissenschaftlicher Rat). This body is composed essentially of the "fellows" of the society (Wissenschaftliche Mitglieder) who at the moment number some 210. The scientific board which, for working purposes, is divided into three sections—biological-medical, chemical-physical-technical and humanities—formulates policy initiatives for consideration by the senate, particularly those concerning the foundation of new institutes and research sections, and also makes recommendations for the election of new fellows. Under the recent proposals for reform both senate and scientific board will be enlarged by the admission of representatives of the non-permanent staff of the institutes. The administrative structure is completed by a general assembly (Mitgliederversammlung), which elects the senate, and an executive board (Verwaltungsrat) elected by the senate and working closely with the president. The president is elected by the senate for a six-year term with one consecutive period of re-election allowed.

Large and Small

The institutes vary considerably in size from the relatively large ones of physics (Munich), carbon research (Mülheim) and psychiatry (Munich) to very small units like those for plant genetics (Ladenburg), cell chemistry (Munich) and physiology of nutrition (Dortmund). The very large Plasma Physics Institute in Garching dwarfs all others with its scientific establishment of 208, but the foundation of such a large institute is quite atypical and does not seem to represent a significant change of policy. The average establishment has some forty scientific staff, though there are large variations. Medico-biological subjects account for a clear majority of the actual institutes founded, though not for a corresponding financial demand. There is some tendency for the institutes to aggregate in certain cities such as Göttingen, Frankfurt and Munich, but this is largely a historical accident and is not a definite policy. Clearly there are advantages in the location of institutes near universities and industrial centres, but these are not regarded as overriding.

It is less generally known that the MPG also services a whole series of institutes outside the natural sciences dealing with special aspects of law, education, patents, documentation, history and criminology. There are, however, certain gaps in the coverage of conventional subjects—sociology as usually understood is not catered for and neither is mathematics, although the society is justifiably proud of its latest creation, the Institute for the Study of Life in the Technological-Scientific World at Starnberg. This centre is "to

coordinate diverse work in the natural sciences, medicine, logic, systems analysis and philosophy in its bearing on all aspects of the environment, world peace, science politics and the problems of developing countries". It is doubtful whether any government supported institute has ever been founded in Europe with such open-ended terms of reference. This group, led by C.-F. von Weizsäcker, is at present engaged in a preparatory phase of their studies, in which specific problems and profitable lines of attack will be distinguished. Other new foundations of recent years include those for solid state research (Stuttgart), biological cybernetics (Tübingen), biophysics (Frankfurt) and radioastronomy (Bonn) as well as the Plasma Physics Institute already mentioned.

Institutes and Universities

Significantly, the titles of individual institutes rarely follow the standard headings of the university curriculum; there is wide coverage of borderline subjects and the chief disciplines of physics, chemistry and biology are spread through many overlapping units with conspicuous mixing of workers from different academic backgrounds. Various forms of joint appointments with universities are possible and, though members of institutes in university cities do not enjoy much contact with undergraduates, they may supervise doctoral students in the normal way by arrangement with the university and themselves submit for their "habilitation" as teachers if they wish.

Although relations between institutes and corresponding university departments are almost invariably cordial, German academics are a little less ready than they were some years ago to see the Max-Planck Society as a purely uncompetitive contender for an adequate supply of government money. Although certain undertakings such as the Plasma Physics Institute and the new radiotelescope would certainly be out of place under university control, there are other recent foundations—the most often cited being the two solid state research institutes at Stuttgart—which, although not in any evident sense tending towards "big science", have nevertheless been funded on a sufficiently lavish scale that it is difficult to believe that university solid state research will not be starved, whether deliberately or indirectly, of some of the federal funds that might otherwise have come its way.

Another symptom of present unease in German scientific circles is that many scientists in the MPG are beginning to express the view that, with the increasing pressure on university staff through student demands and the need to participate more formally in department business, it will become increasingly difficult to carry out effective research in the universities which, they conclude, will eventually be forced into a purely pedagogical role. Although there is obviously an element of self-justification in these views, it is difficult for outside observers to escape the impression that, in the later years of the 1960s, both the objective resources and the morale of university research workers have suffered relative to those of their colleagues in the institutes.

All the same, a healthy degree of interchange between university and institute appointments, in both directions, remains the rule, the predominant tendency being for scientists to move across to the Max-Planck institutes at the immediate post-doctorate level, build a reputation there and, if successful, make a later choice between continuing as an established "fellow" of the society and accepting a call to a chair at a university. The process of university teachers resigning to take a senior position in an institute is still relatively uncommon, so that the institutes have tended to remain a proving ground for their own eventual leaders.

Finance

An important factor in the success of the society in the post-war period has undoubtedly been its very circumspect attitude to the "big machine" science which has so bedevilled policy making elsewhere. In fact, the MPG may provide a

very useful case study on what can be achieved by imaginative planning over extremely diverse areas of "small science", on an income which, though politically considerable, is nevertheless almost trivial by the standard of Concorde, to say nothing of military budgets. Nevertheless, it is important to realize that, although one element in the rationale of the present constitution of the MPG is the need to provide for projects which would be so large as to create instabilities in the university system, its policy of avoiding most really large single items is not simply dictated by the need to husband financial resources and preserve a flexible approach to the federal treasury, but equally because such items would represent a clear threat to the society's long tradition of individuality and even adventurousness in the make-up and direction of new institutes.

Consequences of Individuality

It is around the various corollaries of this policy of individualistic leadership that the recent period of soul searching in the Max-Planck Society has developed. Although, for at least the last decade, the society can be said to have been living down the principle of the one man institute, the fact that it remained committed to it constitutionally seems to have encouraged a minority of institute directors, of the more autocratic school, to resist the quite reasonable aspirations of the junior staff, particularly those in the grade of scientific assistant (*Wissenschaftlicher Mitarbeiter*) who have always performed a large proportion of the benchwork while employed on a limited term basis.

The extremes to which the idea of the one man institute was carried in the earlier days of the society have an almost ludicrous quality now, though this was clearly almost an article of faith among the élite responsible for the original foundation. Whereas in German universities, just as in England, professors formerly ruled their departments by a sort of tacitly accepted divine right, the heyday of the Kaiser-Wilhelm Gesellschaft was characterized by frequent proclamations of the virtue of monarchical rule, no secret being made of the fact that an institute was granted to its director as something in the nature of a personal fief, to be used at his sole discretion, for the furtherance of his chosen field, which he remained at liberty to change as he saw fit. Max Planck himself wrote extravagantly that: "The director is the soul of the institute; he initiates policy under the general statutes and acts as lord of the house over the estate which has been placed at his disposal. With the resignation of the director, the institute loses its goal, and—what would be unthinkable for a government department—may be switched forthwith to other purposes, or closed down, or replaced by another according to requirements and resources . . .".

Attenuated as this principle may have become since the post-war reconstitution of the society, it can hardly have been expected to appeal much to the younger generation of research workers, many of whom were beginning to have experience of more human systems abroad. Thus it was not surprising that the unrest of 1968, though concentrated in the universities, found at least some echo in the Max-Planck institutes. At first the president dismissed criticism with the assertion of the familiar formula that financial independence equals accountability equals autocracy, claiming that most expressions of dissatisfaction came from the non-permanent staff and were therefore negligible—an attitude somewhat inconsistent with the frequent praise bestowed on the "Nachwuchs", precisely these up and coming junior workers, on other occasions. The scientific assistants retaliated by creating their own organization to formulate a policy of reform, and the administration, to its credit, reconstituted a previously appointed presidential commission on questions of structure to allow the junior staff to be represented.

In June last year, a critical point was reached when the

Wissenschaftliche Mitarbeiter, impatient with the structure commission's delay, called a large and well publicized conference at Arnoldshain to launch a number of demands—the now celebrated "Arnoldshainer theses"—in which the need for more collective decision making and limitation to the powers of institute directors was coupled with an idealistic programme challenging the Max-Planck Society to take a more deliberate interest in social and environmental problems. Although the Arnoldshain conference seems to have been something of a rhetorical field day, it undoubtedly had a salutary effect; wide coverage was given in the press, government deputies spoke of the need to make further financial grants conditional on reforms, and no one could continue to assert that the reformist camp was limited to a few agitators ungrateful for the start in scientific life which the society had given them.

This period of uncertainty was broken with the publication, at the end of March this year, of the long awaited report of the structure commission. Although certain of its conclusions were tentative, and there were dissenting opinions from some members, the chief recommendations are certainly radical in the context of what went before and, if finally implemented—the senate has already given agreement in principle—should go some way towards meeting the demands of the reformers. The first reactions of the German press and the scientific world have been cautiously enthusiastic, those of the Arnoldshain faction predictably less so.

Recommendations

At the institute level the commission recommends that the whole scientific establishment, including the scientific assistants and others on fixed term appointments, should play a full part in the general decision making process, including deliberations over new appointments. Although the director of an institute retains a right of veto over the majority decision of his colleagues, he will face the need to justify this publicly, as it were, while the dissenting staff will have the right to publicize their disagreement in the annual report and, if desired, take it to a special review committee (*Fachbeirat*) which will be set up to counter-balance to some extent the power still vested in the director. The *Fachbeirat* will consist of scientists of international standing, some from outside the MPG, and will be charged with regular reviews of the progress, scientific reputation and policy development in the institute with power to take remedial action above the head of the director if necessary. As a further corrective to the misuse of power by directors, it is proposed to discontinue the system of lifetime appointments in favour of a reappointment procedure at fixed intervals, probably of five or seven years. A number of safeguards are also proposed to protect the right of all members to carry out and publish research on themes of their own choice as well as those of the group to which they are attached, and to see that their contribution to all publications is correctly acknowledged. Directors of institutes will also be obliged to interview their non-permanent staff at fixed intervals to discuss their professional situation, if desired in the presence of a witness.

At the higher levels of the society there are also significant concessions to the *Mitarbeiter* grade. It is proposed that their representatives should sit, with equal rights, among the full "fellows" on the scientific board, making up some 30% of the seats. A corresponding proportion would then also be elected to the senate.

The principal criticism of the Arnoldshain reformers has been that an essentially non-voting voice in institute affairs may be as bad as useless under an incorrigible autocrat, the situation where change is most needed. They also point out that the limited contracts will only apply to new appointments and that all present directors will continue for many years with a lifetime's contract in their pockets. Hopes that a rotation of directors might lead to a valuable

"new broom" effect have been discouraged in statements to the press—clearly the administration see the re-election of directors as normal, the review procedure being simply a restraint and a way out in times of real crisis.

Nevertheless, there seem to be good grounds for believing that the proposed reforms will carry far more weight than their critics are willing to admit. Whatever they fail to achieve overnight, they certainly mark the constitutional end of Planck's concept of the omnipotent director and of the very real lack of humanity that went with it. At the same time it is encouraging to realize that the reforms only give substance to what has already been taking place for several years in a number of institutes, particularly where younger directors are involved. Various forms of rotating, collegiate and double leadership have, in fact, been in operation, it seems without full constitutional sanction though with the informal blessing of the senate. There has also been a conscious attempt to divide the really large institutes into virtually self-governing departments, while in other cases several smaller institutes have been encouraged to opt for a system allowing considerable freedom within a joint administration.

At the same time, whereas the retirement of a director can still lead to a period of unease in an institute, it is no longer the traumatic event it was in earlier days. Although the structure commission upholds the principle that flexibility must be preserved, if necessary by the closure or rededication of an institute which has outlived its usefulness (why this necessity should only arise with the retirement of a director was never quite clear), the winding up of a large institute now seems unthinkable and the closure or re-orientation of smaller institutes in recent years has usually been possible by a relatively painless formula of amalgamations or sometimes transfer of units to a university department. The most recent examples of a winding up operation are the late Professor Warburg's cell physiology institute in Berlin and the silicate research institute at Würzburg. Another complete restructuring occurred at the fluid dynamics institute at Göttingen, once directed by Prandtl. Neither is there undue haste in deciding the succession, especially in larger institutes, and a temporary "commissarial" leader is sometimes appointed when the choice is not clear. This system has been in operation since Professor Heisenberg's retirement from the leadership of the institute for physics at Munich, with Léon Van Hove presently in charge, though apparently not on a permanent basis.

After the publication of the reform proposals, one German newspaper gently rebuked the Max-Planck Society for having shown so little readiness during the past few years to apply a scientific approach to its own structural problems. There is perhaps some justice in this observation. Not that they would have been advised to seek over-zealous solutions—rather it could be said that the society should be large enough by now to take an empirical attitude to the question of leadership and decision making. By now, and certainly within the next decade, it should be quite possible to see, in averages over groups within the fifty or so separate institutes, whether, for example, forms of collective leadership do lead to stagnation and mediocrity, whether there is an optimum size for institutes and sub-departments, and whether any price at all has to be paid for dropping the autocrats and the invidious practice of letting whole institutes decline with them into retirement. Whatever conclusions may be reached, one is inclined to think that not only will the Nobel Prizes continue to find their way into the society, but that, to the delight of the new generation, they will occasionally appear at the wrong point in the hierarchy.

Few German scientists among those who wish the Max-Planck Society well in the first days of its new constitution will deny that the present climate of economic uncertainty

in the Federal Republic of Germany is not an ideal one in which to initiate sweeping changes. Already the first budgetary cuts, which pessimists see as an inevitable sequel to the situation in North America and Britain, have begun to be felt and there is alarm if not actual despondency in many university departments. The Federal Science Minister, Herr Leussink, has also cautioned the society that the recent very considerable rate of increase in their grant (24% in 1970–71) might have to come to an end. Nevertheless, it seems likely that the administration, true to its record in the past, will weather the present difficulties by the combination of good housekeeping and continuous review of priorities on which it prides itself. If there are to be financial restrictions, it seems likely that they will lead to some cutting back of plans for the foundation of new institutes and to an early reorganization of others rather than to wholesale austerity.

By the time the society has emerged from this likely period of consolidation and put its proposed reforms into effect it may well bear as little resemblance to its image in the 1960s as this did to that of the Kaiser-Wilhelm Gesellschaft fifty years earlier. The post-war graduate will completely predominate and Reimar Lüst's presidency seems as likely to be influenced by the easy going traditions of the American graduate school as by the fearsome emperor-directors of the past. There is every reason to believe that he will lead the society into its changing role rather than be content to live with the compromises recently worked out.

Ideas for Others ?

Could the Max-Planck system, in something like recognizable form, be transplanted into Britain or elsewhere, given the right political and economic climate? At first sight this would seem a forlorn hope, for even if the whole ethos of the MPG were not so different from that which seems to pervade government supported science in Britain, the grafting of such a complex among so many vested interests would present formidable administrative problems. It is certainly difficult to imagine a country which has difficulty in supporting one Royal Institution—which is perhaps the closest analogy to be found—branching out in support of several dozen. Even so, the difficulties must be seen to be political and administrative rather than economic and there would be few financial reasons why a start should not be made through a new association of existing research organizations. (The research councils could still have a well defined, separate role, just as the Deutsche Forschungs Gemeinschaft does in Germany. Only in France, with the CNRS, do the several functions appear to be rolled into one.) Even with government sympathy, however, such an operation would still have to get over the barrier of cherished obsessions—with "big machine" science, with the idea that pure research must go hand in hand with undergraduate teaching, with the idea that government supported research outside the universities must involve close ministerial control and civil service formality.

This is perhaps an inopportune time to seem to be advocating change to a system which would not simply resist the conclusions of the Rothschild report but actually stand them on their heads. The Max-Planck Society is as near a negation of Rothschild as one could hope to find; it exists, with government blessing, precisely to protect both pure and applied research against both the vicissitudes of economic and academic policy and the threat of ministerial interference. It is the wholehearted acceptance by the German federal government of the need for such protective cover, cutting across all distinctions of pure and applied science and embracing parts of the humanities, which makes the Max-Planck Society the force which it has undoubtedly become in Western European learning.

FRANCE

Crucial Year for French Science Policy

from *La Recherche*

It is becoming increasingly evident that the sixth research plan is not running as smoothly as might have been hoped.

DURING the past few years the evolution of French science policy has been characterized by innumerable pitfalls and an imprecise course. But 1972 may be a crucial year to the extent that once the research budget for 1973 is known, it will be possible to discern the trends of research as scheduled in the sixth research plan (1971–1975), and, especially, to find out whether or not some research agencies will succeed in finding their second wind.

Using the budgets of 1971 and 1972 as a guide, it is now possible to prepare a rough balance sheet for the execution of the sixth plan which, on examination, suggests that the public funds destined for research are falling behind. The average annual rate of growth of research and development expenditure for the two years was 7% (in 1970 francs), whereas the plan anticipated an upturn in the growth of funds from 7.5% to 11%. The growth in 1972 of the programmes which are covered by the plan is large (12.7%) but this is less than the average growth of public expenditure (16.9%). The sectors recognized by the plan as having priority, such as the biomedical and social sciences, were also growing at a smaller rate than was planned. The funds destined for basic research increased but at a smaller rate than the average for 1972. There are also great disparities between disciplines. Fundamental biology, for example, experienced stagnation in 1972 but, by contrast, industrial research, which had been favoured by the plan, grew by 33% between 1971 and 1972.

It must be noted that an important objective of the sixth plan was not attained, namely the levelling off of funds destined for the so-called *grands programmes*. Although the plan forecast, for example, a reduction of the share in the national research and development effort enjoyed by the *grands programmes*, it seems that they resisted the erosion. The programme of research into the production of electricity by nuclear means, which was intended to be maintained at a constant level, grew by 16% between 1971 and 1972.

Everything points to the inability of the sixth plan to attain its objectives, so that expenditure by the end of 1975 will be, at best, the minimum anticipated (19,500 million francs). This situation places the administration charged with the coordination of the science policy in a very embarrassing position, for commitments have been made by certain research bodies like CNES (Centre National d'Etudes Spatiales) and CNEXO (Centre National d'Exploitation des Océans) on the assumption that the rate of growth of funds would be larger. As the 1973 budget is not likely to alleviate the situation, the objectives of the plan in 1973 will certainly have to be changed. The difficulties arise from the *grands programmes*, notably the atomic energy programme and the space programme, both of which place a heavy burden on the plan. The agencies chosen to carry out these programmes, such as CEA (Commissariat à l'Energie Atomique) and CNES, also find themselves confronted with serious structural problems. CNES, for example, has been forced to close its centre at Bretigny and to transfer most of its activities to Toulouse.

Since the decision in 1969 to abandon the French graphite-gas nuclear reactor, CEA has experienced great difficulties in restoring equilibrium. It now hopes to contribute to the conception of the second generation of light water nuclear reactors in order that France might gain some independence from the nuclear industry in the United States. It is actually more a problem of industrial policy than of science policy that faces the CEA in this field; one must acknowledge that the Minister of Industrial and Scientific Development is also hesitating about the direction to take.

In the field of oceanographic research, there is a structure problem; CNEXO had to be from the start a flexible agency responsible for the organization of the very dispersed oceanographic research activities in France. In this respect, CNEXO was unable to resist the temptation to construct its own laboratories and to invest heavily in costly equipment. It was, therefore, recently decided to reinforce the powers of the technical and scientific committee of CNEXO so that it actually functions as a coordinator between agencies. A similar orientation is anticipated for IRIA, an agency which is supposed to perform research in computer science and whose operation is not yet satisfactory.

The Centre National de la Recherche Scientifique (CNRS) which, with the universities, has a responsibility for basic research, is confronted with several problems. The budget of this organization is about 1,200 million francs, a sum which posed a few management problems which have been resolved successfully by the reinforcement of the scientific and administrative direction of the Centre. Moreover, with the establishment in 1971 of the procedure of programmed thematic actions (ATP, a procedure which was reinforced in 1972 with 25 million francs), the management of CNRS has tried to define a more selective policy. This policy has, however, provoked some criticism, especially by the trade unions, because it tends to decrease the power of the partially elected scientific committees. Having demonstrated with ATP that it can introduce a new procedure (and this is one of the goals of the operation), CNRS now hopes to develop research in the engineering sciences. Lying half way between basic research and applied research, engineering has always been neglected in France and CNRS intends to fill that gap. But such an orientation might cause problems between CNRS and the schools of engineering and industry.

The most serious problem which has confronted CNRS as well as other research bodies is that of the recruitment and training of its scientists. The year-long debate has centred on the problem of the thesis, for the reform of the universities in 1968 implies a redefinition of the doctoral diploma. The doctoral thesis in science is submitted after about 5 or 6 years, a period considered by many to be too long. Rather than introduce a single degree of the PhD type (as many had hoped), the reform would still recognize the two types of doctoral theses that now exist: a thesis corresponding to the doctorate of the third cycle (which lasts two years) and one corresponding to the "state doctorate".

French science policy has not as yet emerged from its phase of uncertainty and of moroseness. These feelings are certainly apparent at the level of the institutions in charge of the coordination of science policy—in particular DGRST (the Délégation Générale à la Recherche Scientifique et Techniques). These agencies know that certain decisions are necessary, but they also realize that they lack the means of implementing them.

NETHERLANDS

Proposals for Reforming University Science

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Some of the scientific research at present done in Dutch universities may be transferred to separate institutes if suggestions based on a study by a team of management consultants are implemented.

SCIENCE in the Netherlands, like almost everywhere else, is in a state of transition. Long standing feelings of inadequacy and frustration are now resulting in a series of far reaching reforms that are supposed to change the whole structure of Dutch science.

This movement started years ago, but under the present government things are really happening. When, about a year ago, the cabinet was being formed, the first problem was how to satisfy the five participating political parties, each of which had a key position in securing a majority in parliament. One of the results was the creation of a Minister for Higher Education and Science Policy, a position that was given to Mr de Brauw, a lawyer and businessman.

He picked up where his predecessor—much plagued by student activities—had left off, and approached the situation in a very matter of fact and direct way, producing laws and white papers as if they were insurance policies for cars.

Three fundamental considerations form the basis of his activities. First, the present financial and economic situation of the country is far from rosy, and all expenditure has to be reduced. Second, the growth of the universities has been too uncontrolled and some regulations are needed. Third, science in universities is a very obscure thing: one knows the total cost of the universities, one sees scientific publications by university staff, so one may assume that part of the large sum devoted to the universities is, in fact, being used for scientific work. It is, however, far from clear how much is being spent and, particularly, on what. To these points must be added the loud complaints from university staff to the effect that they cannot possibly do any valuable scientific work because of student demands.

There are already some reports, either complete or being drafted, which recommend how to deal with the situation. First, there was the Posthumus report produced by a former rector magnificus of Eindhoven Institute of Technology, Professor K. Posthumus. He started by analysing the curriculum and the use people make of it in their later lives, and suggested that most people learn far more than they really need. One of his chief points, later stressed by the new minister, was that every student is being educated as if he is to spend his life doing research, whereas in fact only a small percentage of students do. He recommends a course of four years duration (compared with an average of about six and a half years now) for a master's degree, with a possibility for those who want to go into research to stay on for two or more years (as a research student) or for three more years (as doctoral students).

Mr de Brauw has now adopted these ideas, adding a few other suggestions such as a general curriculum that has to be taught at every university, and the remark that it shall be decided by the government how many postgraduate

students are allowed in any given year in any given faculty.

The universities have been stunned by these proposals. Most people from both staff and student circles feel that curtailing the time for study to such an extent is bound to have a very negative effect on the quality of the students; they do not see how they can give a really scientific education. The minister himself has pointed out that he does not really expect them to either. He thinks university education has been far too abstract and, although that may be good for the few that go on doing scientific work, he feels that most students come to the university to learn certain crafts and collect a certain amount of practical knowledge.

Against this background of general university reform, there was an OECD meeting going on in Paris, at which the structure of scientific education and university science was being discussed. At that meeting were a Dutch cabinet minister and a director of Shell (which is 60% Dutch). It was proposed that, on a trial basis, one member country should have its universities checked by a professional efficiency expert, the cost to be borne partly by OECD and partly by the government of the member country concerned. Holland volunteered to be a guinea-pig, especially because the Shell director offered \$18,000 to cover part of the cost of an analysis by McKinsey and Co. of New York, a company with which Shell has had long standing relations.

McKinsey and Co. came up with the idea that only well managed research can be effective research. Good management is not exactly a feature of every university, and there was a fear that any management would become impossible once the new university structure (with students, staff and trade unions represented on the board) took shape.

McKinsey also suggested that there are two types of research: education-bound research and scientific frontier research. The former is, by nature, appropriate to the university; the latter, it said, should be taken out of the university structure and done in extramural institutes.

These institutes would have connexions on a personal level with the universities because the staff of the institutes would lecture there; they would also offer opportunities for graduate students and would be individually financed by the government on the basis of their scientific qualities and programmes and according to the needs of the country.

This structure would really enable the government to carry out a science policy by moving budgets from one institute to the other, and one of the chief reasons for adopting it is the possibility of combining efforts that are now thinly spread over several universities into one institute, which could then more easily become a centre of excellence.

Opinion in the universities on the issue is divided. Many members of staff are attracted by the idea of just doing research without the hassles that go with the rest of university life, others favour the very concept of concentrated research efforts, and still others think that McKinsey has offered a way out of a severe financial problem. On the other hand, many people are opposed to the scheme because they feel the distinction between two types of research lacks any basis—there is only good research and bad research, they say. Good research can be done with the help of students (in fact, it can hardly be done without) and such research is to be moved out of the university sector; that will immediately affect the quality of the graduates and, the argument goes, the quality of research in the longer

run. Some are also afraid the McKinsey structure will lead to a very autocratic, centralized and bureaucratic form of science policy. There would, then, be very little room for new ideas; fashionable topics would probably be stressed at the expense of other fields that draw less public attention.

Especially among leftist students and scientists, one hears the objection that the system would be very undemocratic. They are afraid that any form of scientific work will have to be geared to the interests of industry and other powers in present day society in order that support should be obtained. At present a university department is free to develop medical instruments for North Vietnam or to do research on changing the power structure, but some think that this situation will come to an end with the implementation of the new structure. These ideas got some more support when it became publicly known that Shell had been paying for part of the work that led to the proposed structure. But the storm of protest was short and never reached gale force.

There is little doubt that science in the Netherlands will be reformed according to the present plans. The statement that everything will be cheaper and more effective is bound to strike a chord with the majority in parliament that supports the present government. And even if it does not, it is now unlikely that a stop would be put to the implementation of the proposals.

In its great effort to economize, the government has increased

the cost of attending a university from Dfl 120 (about £14) to Dfl 1,100. Further rises are imminent, probably to about Dfl 3,000 a year. The Dutch equivalent of the House of Lords has turned the proposal down, but the rise is to be effective in the new academic year.

The most protest has been made at a proposal to limit the number of students in different faculties. In the Netherlands, everybody who has passed the final examinations of certain types of secondary schooling has a legal right to study the subject of his choice at the university of his choice for as long as he wants to. In the past, the budget of a university was simply based on the number of students, but that situation is now being radically changed. The universities are not allowed to appoint any new members of staff, and consequently the number of students has to be limited. And it is.

Strangely enough, one gets the impression that the whole set of government measures, which are bound to affect the university system for decades ahead, has not had half as much effect on people as the report, *The Limits to Growth*, which was published by the Club of Rome some months ago. Every department in every university now has at least one working group studying the problems of the environment, and, as one journalist wrote recently, "it is very clear that the exponential growth of the number of hysterical professors constitutes a serious threat to mankind".

CERN

European Particle Physics at Geneva

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Although the intersecting storage rings and the new machine attract most of the attention, the existing facilities still have a vital role to play as does the cooperation with the laboratory at Serpukhov.

THE past year has seen the beginning of a new phase of activity at CERN in Geneva. The intersecting storage rings (ISR) have come into operation, making experiments possible at centres of mass energies far higher than had ever been available before. Also a start has been made on the construction, alongside the established laboratory, of the large new accelerator (called the super proton synchrotron, SPS) designed to yield energies of several hundred GeV (Fig. 1).

Within Europe one can note a new solidarity in particle physics with the participation of all twelve member states in the combined research programmes on the existing accelerators and the ISR, and with the recent decision of Denmark to add its name to those of the ten other countries participating in the new accelerator project. At the same time, collaboration with the Institute of High Energy Physics at Serpukhov in the USSR has gone on apace and the continuing close association with the high energy physics laboratories of the United States has ensured a collaboration of truly global dimensions.

Building the new accelerator alongside the existing facili-

ties (in what is to be known during the construction period as CERN laboratory II), and thus concentrating the principal European machines in one place, has had a number of repercussions. It has meant a very considerable saving in capital outlay, as the eight-year project is now scheduled to cost approximately half of what had been proposed in earlier years. Also, by making use of some existing large detectors, it is envisaged that experiments could start at intermediate energies in the sixth year of the project. But in spite of this considerably diminished capital requirement, the total sums that member states now feel able to devote to high energy physics research are such that a real restraint has been put on the programmes in individual member countries and cuts have been made in the budgets that had been foreseen for CERN laboratory I. The extent of the squeeze is amplified by the additional load put on the laboratory, first in providing facilities for laboratory II and in making preparations for ultimate research with the new machine, and second by accommodating a further increase in the use made by physicists in the member states of the CERN facilities. The restraints on the home budgets inevitably mean that member states seek to benefit even more from the existence of the international laboratory.

To meet this new situation there has been a tightening up all round and intensive studies have been made of the way in which efficiencies can be increased so that there will not be too serious inroads made into the scheduled physics programme. It is clear, however, that the scale of the programme cannot be the same as was envisaged a few years ago, and a very careful look has been taken at the priorities that need to be observed. The policy remains one of maintaining the highest quality consonant with the

provision of the necessary service to the many universities in the member states who are dependent on the CERN machines for the continuation of their research.

The existence of two interconnected laboratories within the same organization has raised several novel administrative problems—for example, how to ensure that the budgets of laboratory II and the cost of the new machine are separately identified while maximum use is made of the existing services of laboratory I. At the international level, the existence of the second laboratory has also raised novel questions. Laboratory I, although half in Switzerland and half in France, can only be entered from Switzerland—the seat of the organization being Geneva. The new machine will be built in a tunnel some tens of metres underground and also across the Swiss-French border, but almost all the surface buildings and the special beam lines will be in France.

It is the wish of the organization and of the two countries concerned that the site should be essentially open with only the area containing the buildings “fenced off”; this means that, on the surface, laboratory II will be separated from laboratory I by open country. The communication problem between the two laboratories is likely to be solved by arranging for an entry from France into laboratory I for CERN staff, a procedure which will establish a precedent in the control of an international frontier. Similarly, the problem of telephone communication between the laboratories and the outside world is being tackled in a highly pragmatic manner. This gives further proof, if such were needed, that in CERN the collaboration is not simply scientific but has its parallel in the administrative or, one might say, political arrangements also.

Work has already begun on the construction of the buildings and assembly hall for laboratory II and on the approaches to the main ring tunnel of the SPS. Vertical shafts are being dug down to tunnel level and, this autumn, the tunnelling machine will be lowered down one of them. The “mole” will be connected to tunnel-lining and spoil-moving machines, making a unit 40 m long which will burrow its way around the 7 km circumference of the ring; this operation will take about 18 months. Detailed specifications are being prepared for the machine components and calls for tenders are going out progressively to European industry. An intensive testing programme has also started, for example, of magnet models for the main ring, to finalize details of the design.

The options that were built into the original programme remain open; that is to say, iron-cored magnets to fill half the ring only will be ordered initially, pending decisions to be taken on the applicability of superconducting techniques to the later parts of the programme. Development work on pulsed superconducting magnets is being pursued principally in three European laboratories: at the Rutherford Laboratory in Britain, at Karlsruhe in Germany and at Saclay in France. These laboratories are in close touch with industry so that the manufacturing technology and the development can go hand in hand. Overall coordination is provided by the GESSS committee (Group for European Superconducting Synchrotron Studies). An advisory committee of outside experts has also been set up to ensure close contact between CERN and the accelerator laboratories of Europe during the construction programme. Earlier this year, this committee organized a theoretical study to examine in depth aspects of anticipated beam behaviour in the machine.

Planning Experiments

It is standard practice that proposals for experiments on the existing accelerators and the ISR are submitted to committees which meet under the chairmanship of senior scientists from outside CERN. Most of the research is carried out by visiting physicists who remain based at their home



Fig. 1 Aerial view of the CERN laboratories with the outline of the SPS main ring. The future beam line and laboratory buildings are shown dotted alongside the established laboratory. The crosses indicate the Franco-Swiss border.

research laboratories and visit CERN, generally for fairly short periods. The experimental committees ensure that the proposals for new experiments are considered by representatives of the whole of the European physics community. In the same spirit, work has already started on the preparation of the experimental programme for energies of several hundred GeV so that the machine design can, from the beginning, be influenced by the research foreseen.

Under the auspices of ECFA (European Committee for Future Accelerators), which played an important role in formulating the recommendations which led first to construction of the ISR and then to the SPS, a special working group has been established to ensure liaison between the machine builders and the experimental community. At a plenary meeting in Tirrenia in the autumn of last year, ideas were put forward which resulted in significant modifications to the proposed experimental facilities. Thus as well as the formal links with the member states through the CERN council and with the senior scientists through the scientific policy committee, there is a means of consultancy and communication at several levels in the experimental and construction programmes.

This intercommunication at all levels has become all the more necessary with the increasing dependence of physicists in the member states on the CERN facilities. It has been no less important in establishing with such speed the experimental programme on the ISR, which has now been in full operation for a year. This machine which, in Britain particularly, caused great misgivings when the design study was published in 1964, has performed better than anyone could have dared to hope, and has also proved to be a very versatile experimental tool.

Beams were stacked and stored in the two rings for the first time in January last year and almost at once experiments began. The smoothness of the commissioning and the final switch-on, coupled with the fact that the machine was completed ahead of schedule and within the original budget estimates, ensured that the experimental programme got off to a flying start. There is still a great deal to be learned about beam behaviour, but beams of more than 10 A can now be stacked and stored. With beam currents of 6 A, the decay rates are very low and the background in the intersection regions (Fig. 2, for example) is much smaller than had been anticipated. The stability and pre-

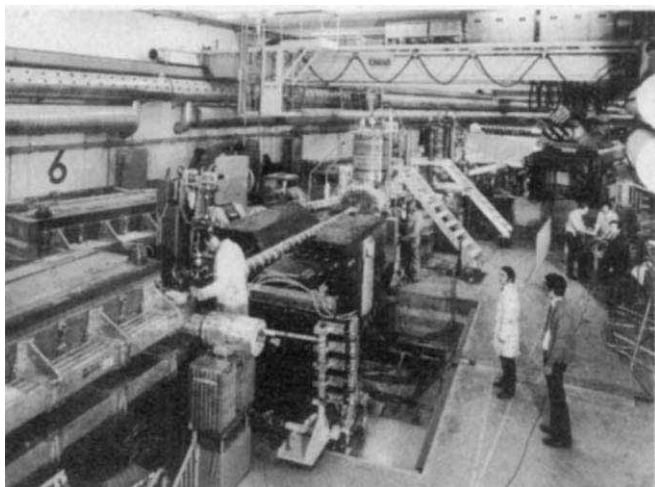


Fig. 2 An intersection region of the ISR in the absence of experimental equipment.

cision of the magnet system are of a very high order and the delicate manipulation of r.f. fields is a remarkable piece of gymnastics, but both maximum current and the beam life depend ultimately on the quality of the vacuum in the system. Whereas a mean pressure in the rings of 10^{-9} torr had been specified in the design (a pressure then regarded as at the limits of vacuum technology), mean pressures nearer 10^{-10} torr have been achieved and in the intersection regions about 10^{-13} torr.

Results Achieved

The experiments at the new energies available at the ISR have not yielded evidence of any of the particles whose existence has been postulated for several years. The quark, the intermediate boson, and the Dirac monopole remain as elusive as ever. Nevertheless there have been a large number of intriguing results and yet again extrapolations from present knowledge at lower energies have proved to be incorrect.

In elastic proton-proton scattering a considerable amount of data has been gathered. The cross section falls off steeply with angle and the rate is proportional to the square of the proton radius. In the lower energy regions the proton radius (or its region of influence) has been shown to grow steadily as the energy of the interaction increases. Surprisingly the growth is smaller than is predicted by the very successful Regge theory from the Serpukhov results at the highest ISR energies and there is a striking change in the rate of fall-off at small momentum transfers. No explanation for this observation has yet been forthcoming.

A successful method of looking at particle production at the ISR has been to consider the interactions as "inclusive interactions"—in which an outgoing particle of a particular type is recorded regardless of what else goes on in the interaction. There are theoretical predictions that the cross sections for inclusive interactions will be independent of energy at very high energies if the cross section is expressed as a function of the proper variables. Results so far are close to the predictions. At the same time, observations of particle production have thrown up surprises such as a much higher rate of antiproton production than was anticipated. This again awaits theoretical explanation.

Because of the complexity of the whole field of particle physics, there is no diminution of interest in research at the lower energies, and each energy range appears to have its own mysteries which need unravelling. The collaboration with Serpukhov has been a particularly valuable addition to the CERN facilities. Already two important experiments have been performed together with Soviet scientists on the

76 GeV machine at Serpukhov, and two others are on the way. Shortly, these experiments will be supplemented by photographs taken in the hydrogen bubble chamber, Mirabelle, taken to Serpukhov from Saclay. According to the CERN-Serpukhov agreement, CERN was to supply a fast ejection system and a separated beam line. These components have now been successfully tested and have produced a separated beam of 32 GeV kaons for Mirabelle—the highest energy available in a separated secondary beam anywhere in the world. The collaboration with Serpukhov has proved extremely fruitful not only in providing new scientific data but also in fostering close contact with the particle physicists of Eastern Europe. Considerable satisfaction has been expressed on both sides with the warmth of the collaboration and its effectiveness.

Other Facilities

Exploitation of the CERN 28 GeV proton synchrotron (PS) is greater than ever. Fortunately, the machine performance has been improved in parallel—over the past year, average accelerated intensity was 2×10^{12} protons per pulse and slow ejected beam efficiencies reached 90%. An extensive facelift for the PS is almost complete. An 800 MeV booster, inserted between the 50 MeV linac and the PS ring, is being commissioned and should make it possible to achieve beams of 10^{13} protons per pulse. Among the new detectors is the heavy liquid bubble chamber Gargamelle (12,000 l.) which was inaugurated in May 1971 and has since completed some long runs in neutrino and antineutrino beams with the chamber filled with freon. It is at present performing a series of experiments with the chamber filled with propane and excellent pictures are being obtained. The number of neutrino events which are being collected per proton ejected from the PS is more than an order of magnitude greater than was possible with the old CERN heavy liquid chamber.

At CERN's smaller accelerator, the 600 MeV synchrotron, the accent is on nuclear physics, with research taking place on such phenomena as very short lived isotopes and mesonic atoms. This accelerator is to be shut down shortly for the installation of a new r.f. system and ion source which will raise the internal beam current from $1.3 \mu\text{A}$ to $10 \mu\text{A}$ and so give the machine, which has been heavily used since 1957, a new lease of life.

An important addition has recently been made to the CERN computing facilities with the acquisition of a CDC 7600 which has become the principal machine of a new central computer complex. At the same time, there has been a considerable development in the systems approach to the use of the machine to ensure its maximum utilization. Input is by way of a CDC 6400 to which will be added shortly a CDC 6500 which has been on the site for some years. These will be coupled to a number of remote input-output stations, some of them equipped with interactive graphical display units. Computers have become an integral part of virtually all experiments whether they involve bubble chambers or electronic detectors.

Automatic scanning and measuring techniques for bubble chamber film have continued to develop but, as most of the photographs taken at CERN are analysed away from the site, it has been essential that standard procedures be established and considerable effort has been devoted to coordinating the techniques throughout Europe; this even has film from the Mirabelle chamber at Serpukhov.

The scientific compulsion to collaborate is manifest at many levels. Collaboration is needed to build the machines in the first place. Collaboration is necessary in the planning of the experimental programme and then in carrying it out. Collaboration is required in the design of experimental equipment and in data analysis. And the collaboration is not confined to Europe but extends to every country which has a developed high energy physics tradition.

DRAGON PROJECT

European Cooperation on High Temperature Reactors

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Experience with Dragon shows that European cooperative projects can be fruitful if their objectives are specific and if sufficient executive power is given to them.

WORK on a nuclear reactor concept involving the use of helium as a reactor coolant and a core consisting entirely of graphite and ceramic materials was started at the Atomic Energy Research Establishment, Harwell, early in 1956. The importance of this type of reactor lies in its ability to operate at very high gas temperatures, its considerable inherent safety and the promise of favourable economics. At an early stage it was apparent that the concept would be a front runner in the competition to develop an advanced thermal reactor. The United Kingdom Atomic Energy Authority, at the time, was heavily involved in the early exploitation of the Magnox gas-cooled reactors, was embarking on the development of a successor to this, the advanced gas-cooled reactor, and had already committed considerable resources to the development of the fast breeder reactor. In the circumstances, the future of the high temperature gas-cooled reactor (HTR) was linked to the possibility of the UKAEA entering into an agreement with a number of European partners to develop the system jointly.

European Cooperation

The proposal to collaborate in a project to develop the HTR was submitted by the representatives of the United Kingdom at the first meeting in March 1958 of a top level committee of the OEEC (now the Organization for Economic Cooperation and Development, OECD) on cooperation in the reactor field. After a careful technical appraisal of the proposal the committee recommended its adoption as the basis for a joint European project, and on March 23, 1959, the agreement setting up the OEEC High Temperature Reactor Project (DRAGON) was signed in Paris by representatives of the United Kingdom Atomic Energy Authority, the Republic of Austria, the Danish Atomic Energy Commission, the Commission of the European Atomic Energy Community (Euratom), the Norwegian Institutt for Atomenergi, the Aktiebolaget Atomenergi of Sweden, and the government of the Swiss Confederation.

The Dragon agreement came into effect on April 1, 1959, and covered a period of five years. The technical objectives involved the design and construction of a 20 MW (thermal power output) experimental helium cooled reactor and an associated programme of research and development in high temperature gas-cooled reactor technology. The conceptual design of the reactor had been described in a paper presented to the Second Conference on the Peaceful Uses of Atomic Energy, Geneva, 1958 (ref. 1). The project was based at the Atomic Energy Establishment at Winfrith and the experimental reactor was constructed on that site.

The original financial basis of the agreement was that the

combined contribution of the signatories should be £10 million, approximately 87% being shared equally between Euratom and the UKAEA, and the remainder coming from the other five signatories in the ratio of the contributions to the OEEC made by those countries. The best assessments of the cost of carrying out the five year programme had already indicated in 1958, however, that this sum would not be sufficient, and the UKAEA agreed to pay an additional amount up to £3.6 million, as necessary, in return for which the reactor experiment and other capital assets would be acquired by the authority on the termination of the project. This arrangement was superseded by a new agreement which came into force in 1962 when the duration of the Dragon project was extended to March 31, 1967, with a total budget of £25 million to which Euratom and the UKAEA continued to contribute 87% with a small adjustment which made the former the principal contributor. Subsequently, the agreement between the signatories of the Dragon project has been extended for two further periods of three years; the present extension runs out on March 31, 1973, when the accumulated budget will stand at £38 million (ref. 2).

Government and Management

The Dragon project is governed by a board of management which meets three times a year, and a general purposes committee which convenes on four occasions a year. Each is composed of representatives of all the signatories to the agreement. It is the duty of the board of management to approve each year the programme of work carried out by the project and the corresponding budget. The board has also been responsible for approving in advance the programme and finances of each successive extension of the Dragon agreement. The general purposes committee has, as one of its chief tasks, the approval of all competitive contracts above £20,000 and non-competitive contracts above £10,000. Below these levels authority for placing contracts is vested in the chief executive.

The project has no legal personality and all necessary legal actions, including the formal placing of contracts, is carried out by the UKAEA on behalf of the project.

The management of the project is conducted by the chief executive and division heads, and their deputies or branch heads. These senior staff are appointed by the board of management, but below these levels the appointments are made by the chief executive. The project itself does not, however, employ its own staff directly. All the professional staff and most of the administrative and auxiliary staff are employed by the national establishments or industrial organizations of the signatories and are seconded to the project, the minimum period of secondment to the project being two years. These personnel are paid by their parent organizations which in turn are reimbursed by the project.

The construction of the Dragon reactor involved the placing of a large number of contracts for plant and materials. These contracts were awarded on a strictly competitive basis following tender action embracing appropriate industrial organizations throughout the signatory countries. Much of the research and development programme of the project has also been

conducted extramurally under contract. These latter contracts have not normally been placed on a competitive basis but have been distributed as fairly as possible between the establishments in all the signatory countries, having regard for the technical competence and facilities available to carry out the work. The professional staff of engineers and scientists seconded to the project have been drawn from all the signatories. An informal rule has always been applied whereby approximately half of the professional staff are seconded by the UKAEA and the remainder from the overseas signatories. As a result of this, several hundreds of scientists and engineers from abroad have come to work on the project at Winfrith for a period of two years or more, during which time their families have been domiciled in neighbouring villages and towns of Dorset and Hampshire.

The part played by the Nuclear Energy Agency of OECD (formerly OEEC) has been important. Having been initially responsible for setting up Dragon, which continues to carry the letters OECD in its title, the agency has assisted in drafting the successive agreements to the project and has supervised the legal arrangements associated with the signing of these agreements. The OECD is not a signatory to the Dragon agreement, but has an observer status on its board of management and general purposes committee. Moreover, the annual reports of the Dragon project are submitted to the OECD Nuclear Energy Agency and published by that organization in English and French.

The arrangement of the Dragon project has proved to be extremely effective. The organization has been divorced from national politics and the involvement of politicians has only become necessary when extensions to the project are being considered. The board of management and general purposes committee have at all times been primarily concerned with the success of the project as a whole rather than in advantages to the particular signatory represented. Sufficient authority has been given to the project management to enable it to proceed efficiently and effectively and reference to the two governing bodies for sanction to proceed with both major and minor work is a simple and speedy process.

Progress of the Project

The construction of the Dragon reactor was started in the spring of 1960 and was finally completed in the middle of 1965. The actual loading of fuel, however, took place in August 1964 and criticality was achieved on the twenty-third of that month. A high proportion of the plant was fabricated overseas while the assembly on site was carried out by a British contractor under the supervision of the engineering group of the UKAEA acting under contract to the Dragon project.

By the end of 1965 the Dragon reactor was operating at 50% of its nominal power and early in the following year the

output went up to the full 20 MW. It is interesting to compare this with the corresponding timetables of the two contemporary HTRs which were built in Germany and in the United States. Site work on the AVR pebble bed reactor at KFA, Jülich, was commenced in November 1960, criticality was achieved in August 1966 and full power in February 1968. In the case of the Peach Bottom reactor, designed and built by General Atomic for the Philadelphia Electric Company, the contract was awarded in 1959, construction started in February 1962, and full power was achieved in May 1967. This comparison indicates, at least, that nothing was lost in the international collaboration but that on the contrary the Dragon project was able to beat its rivals in providing the world's first high temperature gas-cooled reactor. In fact, it is fair to point out that both the German and American groups also derived benefits from international collaboration. The German group, of course, was directly associated, through Euratom, with the Dragon project and maintained very close contact with the latter. In addition, a formal agreement between the United States Atomic Energy Commission (USAEC) and the Dragon project provided for an exchange of information relating to the work carried out on the Dragon and Peach Bottom reactors. This agreement was broadened to include much basic work on high temperature gas-cooled reactor technology and the exchange has undoubtedly been of considerable mutual benefit.

While the construction of the Dragon reactor was in progress, the project conducted a considerable programme of research and development on the general problems of HTRs and particularly the specific problems encountered in the design of its own reactor. When the Dragon reactor experiment had been completed and was operating, it became the principal tool for establishing the basic technology required for the application of this type of nuclear reactor in commercial electrical power generation—for example, for studying the behaviour of materials and possible designs of fuel elements under service conditions. In parallel with this work the project engineers have carried out design studies and economic assessments of large civil power stations incorporating the high temperature reactor. These have subsequently formed the basis for designs adopted by the nuclear industry in preparing proposals for commercial HTR power stations.

Assessment

The Dragon project has successfully achieved its technical objectives of designing and constructing an experimental high temperature gas-cooled reactor, in establishing all the important principles of the underlying technology, and in demonstrating beyond doubt the viability of this system. It has done so at minimum cost to its individual signatories, far less than any one of them would have been required to spend if working alone on the problems.

To some extent the fact that two national groups have also been working on the HTR has resulted in duplication of effort, but even in this situation unnecessary repetition of work has been avoided by the existence of close contacts and a fairly free exchange of information between the three parties concerned. The Dragon project has subsequently also collaborated closely with national organizations and industries in the signatory countries, following up the further development and commercial exploitation of the system. The experience of the project has shown that provided the objectives are sufficiently specific and clearly defined, and that the organization is simple, closely coordinated, and endowed with adequate executive power, an international collaborative programme will be successful.

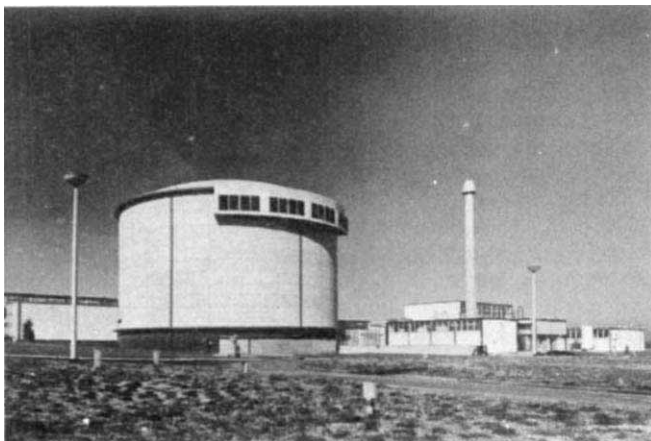


Fig. 1 The Dragon reactor.

¹ *Proc. Second Conference on the Peaceful Uses of Atomic Energy*, Geneva, A/Conf 15/P/314 (IAEA, 1958).

² *Annual Report of the OECD High Temperature Reactor Project (DRAGON)*, Nos. 1 to 12 (OECD, Paris).

A Continuous East-West Fault on the Azores-Gibraltar Ridge

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A continuous east-west reflector has been observed with long range side-scan sonar, extending along the Azores-Gibraltar Ridge from Santa Maria Is. to a point 400 km to the east. From the continuity, narrowness and straightness of the feature, from the evidence of seismic reflexion profiles crossing it and from other published geophysical evidence about the Azores-Gibraltar Ridge, the reflector is interpreted as a transcurrent fault.

THE Azores-Gibraltar Ridge is the boundary between the Eurasian tectonic plate (which includes the north-east Atlantic north of about 38° N) and the African tectonic plate (which includes the north-east Atlantic south of about 36° N). It was initially defined by the line of earthquake epicentres linking the Mid-Atlantic Ridge at 39° N to the Mediterranean earthquake belt rather than by the existence of a topographic ridge. An analysis of the triple junction at the Azores suggests that, relative to the North American plate, the spreading rate of the Mid-Atlantic Ridge south of the Azores is lower than that north of them, implying a dextral transcurrent movement of 0.23 cm yr^{-1} along the ridge¹. The situation is made more

complex by the possibility of a secondary spreading axis running WNW-ESE through Terceira Is.

From fault plane analyses of some of the earthquakes along the Eurasian-African plate boundary, McKenzie² has concluded that at the eastern end of the Azores-Gibraltar Ridge there is a component of compression between the plates; in the central region there is dextral strike slip; and at the western end near the Azores there is dextral strike slip with a component of dilatation. Banghar and Sykes³ have also determined a dextral strike slip solution on the western end of the ridge on the assumption that the fault plane runs east-west instead of north-south. These movements are consistent with an anticlockwise rotation of the African plate with respect to the Eurasian plate, and with the triple junction analysis at the Azores.

The topography of the Azores-Gibraltar Ridge has, however, not been well defined because of a lack of good survey data and because of its complexity, especially in the central and western portions. There is no regional elevation which suggests that it is an axis of seafloor spreading as in a mid-ocean ridge, nor has there been evidence of the existence of long straight ridges and trenches of the kind which typify the major fracture zones. At the eastern end the topography is dominated by massive seamounts sometimes reaching close to the sea surface and grouped into the shape of a slanting "E" (Fig. 1). The line of earthquake epicentres appears to pass through the central arm. Evidence from deep sea drilling⁴ and from seismic reflexion profiles (X. Le Pichon, personal communication) on Gettysburg (Gorringe) Bank (37° N, 11° W),

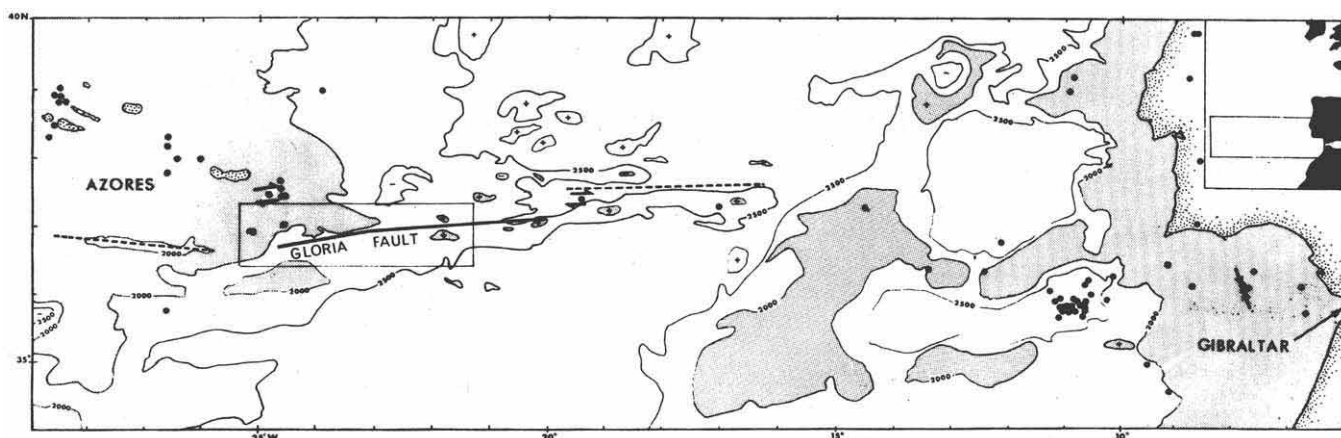


Fig. 1 Outline bathymetry of the Azores-Gibraltar Ridge showing 2,000 and 2,500 fathom contours and earthquake epicentres (1941 and 1960-1970, from USCGS). Three fault plane analyses^{2,3} are indicated. (a) Near Azores; principal movement is dextral strike slip, with some normal faulting (implying extension) with uplift to the north (arrows aligned with slip vector). (b) At east end of Gloria Fault; as (a) but with only slight normal faulting. (c) Near Gibraltar; mainly reverse faulting (implying compression along the direction of the arrows). The solid line denotes the Gloria Fault and the dashed lines related faults to the east and west.

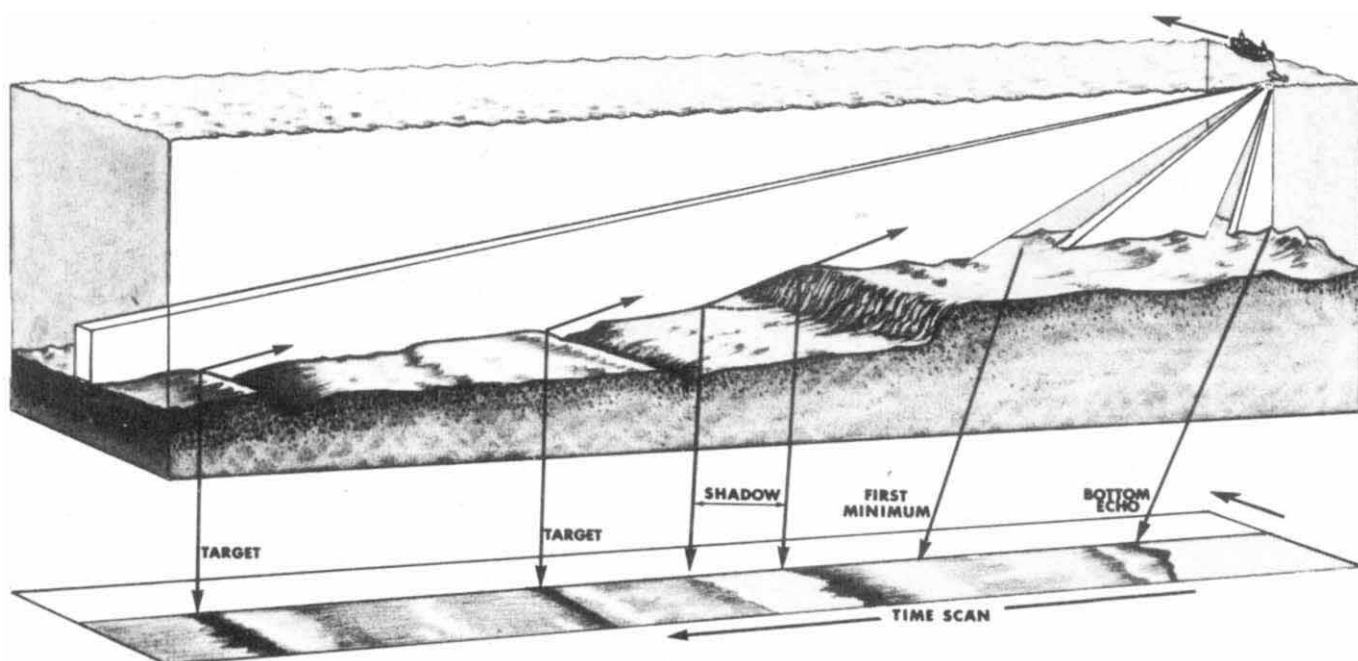


Fig. 2 The principle of operation of the side-scan sonar.

on the central arm, suggests that the seamounts are blocks of ocean crust uplifted in Miocene times as a result of compression.

Between 16° W and the Azores the topography is completely different. There were known to be many smaller ridges and valleys of the order of 20 by 50 km, and no obvious large scale trends similar to those further east. This area was chosen for survey by RRS Discovery in November 1971 because of its importance in understanding the nature of faulting associated with the plate boundary. The long range side-scan sonar system, GLORIA, was used because of its ability to view features of the appropriate scale, and in order to test its effectiveness in covering a substantial area of seafloor with overlapping sonographs. The results of this survey will be described elsewhere. Here we report on a remarkable linear feature found on passage to Santa Maria from the survey area.

The GLORIA system⁵ views the sea bed acoustically from the surface, mapping the oblique back-scattering of a 6.5 kHz pulse out to a range of 22 km on one side of the ship's track (Fig. 2). Experience has shown that reflexions are given by rock outcrops and cliffs, and that contrasts in echo strength arise from textural differences and changes of slope. Strong reflexions mark dark on the record, and shadows, where no

sea bed can be detected, because of slopes dipping away from the ship at angles greater than the incident acoustic beam, show as white patches against the back-scattering from the surrounding seafloor. The beam angle in the horizontal plane is 2°. At a range of 22 km, the beam detects targets along a line 700 m long parallel to the ship's track, although the resolution in range is 50 m. Enough energy is radiated vertically to give a profile of the sea bed. At distances greater than 10 km, the slant range is a close approximation to the horizontal range. The distortion at closer ranges is indicated in Fig. 3. Two displays at approximately 1:1 (true scale) and 3:1 exaggeration of range against distance along the track are recorded as continuous strip sonographs.

On the westward passage to Santa Maria (Fig. 4), the sonar was looking southwards and mostly down slope, and revealed a narrow linear echo which could be followed almost continuously for 400 km (Fig. 3). In places the line bifurcated, in places it divided into several separate parallel strands and in one section (01/322–04/322) it was lost in a large shadow. The echo, when it was single, represented a target not more than about 200 m wide. Over the eastern 200 km, the line did not deviate from a straight line (trending 265°) by more than 2 km, except in short segments (for example, 17/321–

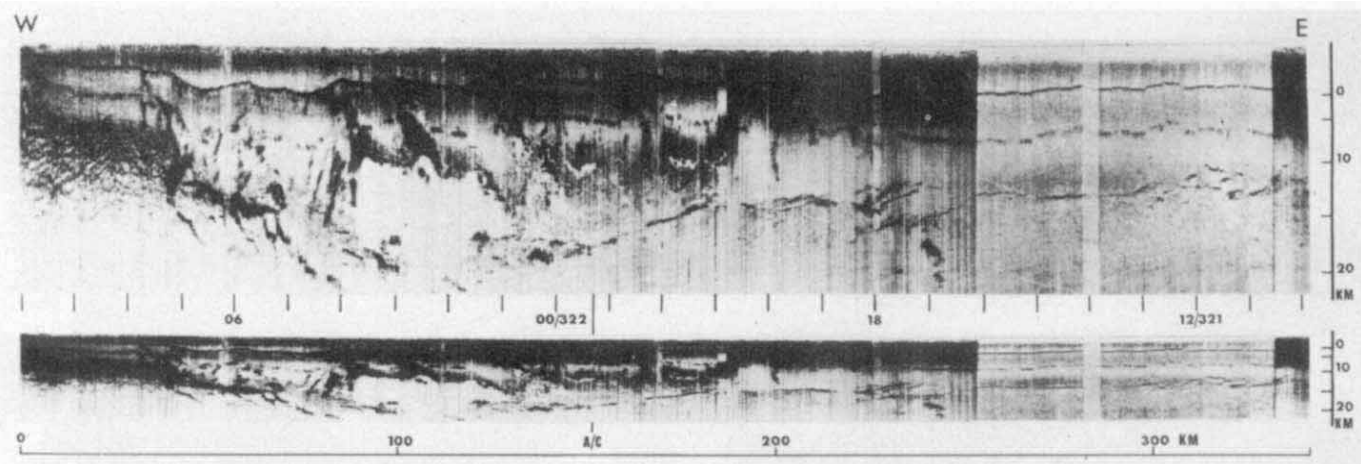


Fig. 3 The sonograph of the Gloria Fault. The upper display has a 3:1 exaggeration of range versus distance along track while the lower display is at approximately true scale (note the non-linear range scale). There was a course alteration at 2320/321 of 7°. The bottom echo and its first multiple are seen near the top of each display.

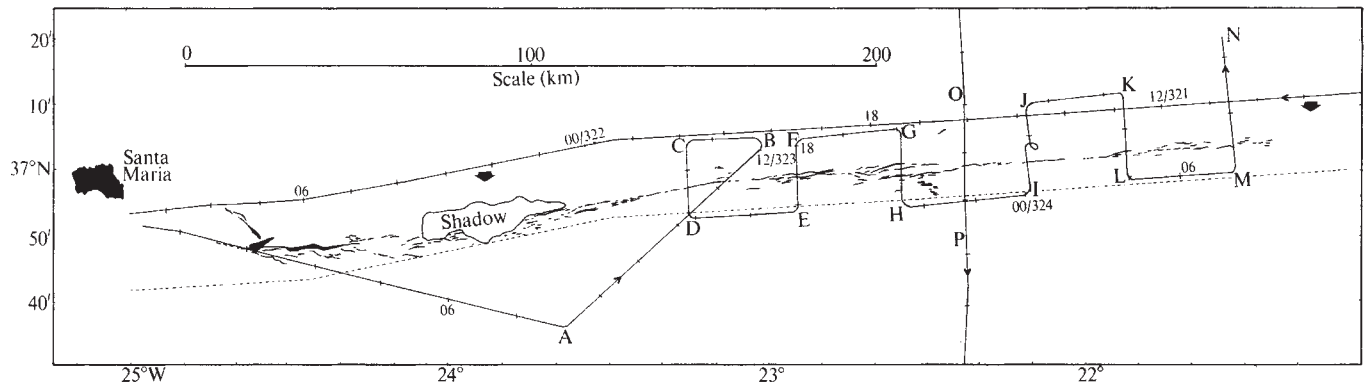


Fig. 4 Track chart and trace of Gloria Fault echoes from the sonograph. Profiles across the fault are shown in Fig. 5. The area viewed by the side-scan sonar along the east-west track is indicated by the arrows and the dotted line.

18/321). We found a second linear trend of 257° west of the position at 21/321. West of the position at 00/322, a series of massive ridges trending 295° was crossed. This is the trend of the island chain of San Miguel, Terceira and Graciosa and the intervening deeps, and also of the islands of San Jorge and Fayal. Krause and Watkins¹ interpret these trends as arising from a spreading centre through Terceira, and explain the deeps as part of a median valley.

On the return track from Santa Maria, after the GLORIA towed vehicle had been recovered, seven N-S sections were made across the feature with the seismic profiling system (Fig. 5). These revealed that the sonar target in all cases had been the north facing escarpment of a V-shaped valley varying in width from 2 to 6 km at the top, and in depth from 100 to 500 m below the regional depth to the south. It is difficult to estimate the local slopes of the escarpment because the echo sounding record shows many overlapping hyperbolae. It is likely that there are several steep cliffs within the 200 m wide zone giving rise to the narrower echoes seen on the sonographs. In places broader echoes, or groups of echoes, were seen from several facets of the north facing cliff. The valley width, however, could only be measured from the N-S traverses.

The continuity of this valley can be inferred from the continuity of the sonograph reflexion. In the western half of the area, the valley occurs immediately to the south of a 1,000 m high scarp separating a ridge, trending east from the Azores, from an area of 500 m thick sediments on the south side. In the eastern half it divides two areas well covered by sediments and more nearly at the same depth. On all the crossings, however, the depth north of the valley was less than that to the south. The sediment layers appear to outcrop on the south side of the valley in the west and on both sides in the east. The valley floor has a rugged topography with no appreciable infilling of sediment, indicating its relative youth. The depths along the axis are not continuously graded, so an erosional origin, as in the case of deep sea channels, is ruled out. We therefore conclude that the feature is a fault. We propose that it should be named the Gloria Fault, since it was only by the use of this long range side-scan sonar that it could be mapped in the detail presented here.

In the west there is a relative vertical movement of about 1,000 m upwards on the north side, which is not apparent in the eastern sections. The great linearity of the feature suggests considerable transcurrent movement resulting from a stress pattern in the crust or lithosphere on a corresponding scale. The sense and displacement of movement along the fault cannot be determined from the data presented here, although it may be possible to determine these parameters from the displacement of the magnetic anomaly pattern if this can be recognized. A preliminary interpretation of magnetic anomaly profiles on either side of the fault does suggest that the Gloria Fault is the major transcurrent fault in this area. In the detailed survey area at 38° N 20° W, we found a well defined NNE trend of magnetic anomalies. Our data,

and that of Williams and McKenzie⁶, show from scattered tracks that the same trend exists, apparently without offset, as far south as the Gloria Fault. South of the fault the track control is less good but it is clear that the magnetic anomalies do not cross the fault. The same trend may be discerned, however, on the south side to at least $36^\circ 20'$ N. The Gloria

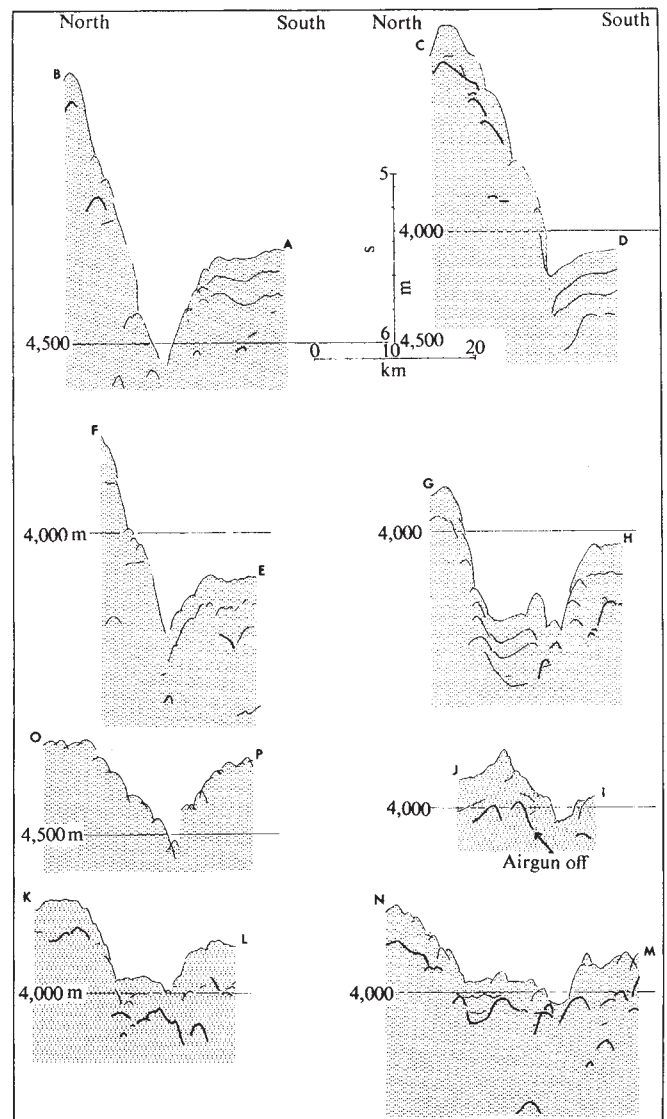


Fig. 5 Profiles across the Gloria Fault traced from seismic reflexion records. Profile positions are given in Fig. 4. Vertical exaggeration 25:1. Acoustic basement is denoted by the heavy line.

Fault terminates the magnetic trends in all cases, strongly suggesting its transcurrent nature.

East of the area depicted in Fig. 3 the Gloria Fault enters a broader flat-bottomed valley closed to the east in the survey area, and cannot be traced further. The trend continues eastward, however, to 16° W in another valley 70 km further north. If the line of the fault is extended westwards beyond Santa Maria, it can be associated with the east-west ridge surveyed by GLORIA in 1969 (ref. 7). The total length of the mapped east-west linear features of the Azores-Gibraltar Ridge would then be 1,100 km, although the topographic expression varies along its length.

No indication of another linear feature of comparable dimensions can be seen on the National Institute of Oceanography bathymetric chart or from bathymetric profiles, confirming our conclusions from the magnetic data that Gloria Fault is the major transcurrent fault at this plate boundary. But in the vicinity of Gloria Fault, numerous small faults with a normal component can be seen displacing the whole sedimentary layer on the seismic reflexion profiles.

The epicentre distribution along the Azores-Gibraltar Ridge shows earthquakes near the Azores and east of 20° W (Fig. 1). The absence of recorded earthquakes in the central region may be a result of the stress being released in a series of earthquakes too small to be observed at the continental seismological observatories. Alternatively, insufficient stress has built up

since the magnitude 8.3 earthquake in 1941 at about 37.5° N, 18.5° W (ref. 8). The solution of a fault plane analysis for a strike slip earthquake is ambiguous, depending on the choice of two possible mutually perpendicular strikes of the fault. We have presented evidence of the east-west strike of the fault, which resolves the ambiguity of the solution near the Azores discussed by Banghar and Sykes³. This solution, also presented by McKenzie², indicates chiefly strike slip with some dilatation and upwards vertical movement to the north of the fault, in agreement with our observations. The solution of almost pure dextral strike slip at 19° W agrees with the lack of relative vertical movement across the fault at this longitude.

We thank all those involved in the running of the GLORIA and seismic profiling systems, and the master and crew of RRS Discovery for cooperation.

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Modification of Morphine Withdrawal by Drugs interacting with Humoral Mechanisms: Some Contradictions and their Interpretation

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Atropine, *para*-chlorophenylalanine and indomethacin modify some effects of morphine withdrawal in the rat. Factors determining the intensity and even the direction of this modification include the particular withdrawal effect observed and the time in the course of dependence when the modifying drug is given. These findings suggest that morphine dependence is multipartite and that acetylcholine, 5-hydroxytryptamine and prostaglandin(s) play intimate parts in various elements of dependence.

PHYSICAL dependence can quickly be induced in the rat¹ and mouse² by continuous exposure to morphine. The degree of this acute dependence may be assessed by the intensity

of the abstinence effects or by the dose of naloxone required to precipitate an effect of given intensity². This gives an opportunity to study the influence on this type of morphine abstinence syndrome of drugs that deprive cells of endogenous humoral substances; but different investigators have reported conflicting results with the same drug. For example, Grumbach¹ finds that atropine intensifies and physostigmine depresses morphine withdrawal excitability in the rat, whereas, according to Fuentes³, atropine lessens and physostigmine increases the intensity of the abstinence syndrome in this species. Again, Way and colleagues² have reported that, in morphine-dependent mice, *para*-chlorophenylalanine (PCPA), which depletes brain 5-hydroxytryptamine^{4,5}, lessens withdrawal jumping; but others^{3,6-9} have been unable to obtain this effect.

Results have also been contradictory when a drug that interacts with a humoral mechanism has been tested against several distinct morphine withdrawal effects. For example, diethyldithiocarbamate, which increases brain dopamine and decreases brain noradrenaline¹⁰, lessens the incidence of the body-shakes and diarrhoea of morphine withdrawal in the rat, while not lessening that of ptosis, and even intensifying the hyperthermia⁷.

Table 1 Abstinence Signs in Rats after 24 h Exposure to Morphine in a Sustained-release Preparation

Abstinence sign	Nx, 0.25		Proportion of animals positive Nx, 0.5		Nx, 1.0		Total		Significance of total T vs. C (<i>P</i> value)
	T	C	T	C	T	C	T	C	
Jumping	13/16	0/17	13/16	1/16	12/13	0/14	38/45	1/47	<0.001
Teeth chattering	15/16	0/17	15/16	2/16	12/13	1/14	42/45	3/47	<0.001
Irritable to touch	6/16	2/17	8/16	1/16	1/13	1/14	15/45	4/47	<0.004
Irritable to handling	10/16	8/17	12/16	4/16	3/13	3/14	25/45	15/47	<0.02
Diarrhoea	16/16	5/17	16/16	5/16	12/13	3/14	44/45	13/47	<0.001
Chewing	15/16	6/17	16/16	6/16	13/13	3/14	44/45	15/47	<0.001
Ptosis	15/16	6/17	14/16	6/16	10/13	4/14	39/45	16/47	<0.001
Body-shakes	3/16	1/17	5/16	1/16	3/13	0/14	11/45	2/47	<0.006
Head-shakes	7/16	6/17	7/16	4/16	7/13	4/14	21/45	14/47	=0.07
Paw tremor	2/16	0/17	2/16	1/16	3/13	1/14	7/45	2/47	=0.07

Test animals (T) received subcutaneously 150 mg/kg of morphine, suspended in an emulsion in paraffin, of an aqueous solution of 0.9% NaCl to form a sustained-release preparation. Control animals (C) received the emulsion without morphine. Twenty-four hours later, all animals received naloxone (Nx) subcutaneously at 0.25, 0.5 or 1.0 mg/kg.

In an attempt to resolve these and other contradictions, the propositions were advanced¹¹ that the intensity or direction of action of a modifying drug on a morphine withdrawal effect may be determined by (1) the particular feature of the abstinence syndrome observed or (2) the time in the course of dependence when the drug is given. To test these propositions, to help resolve conflicting findings in the literature and to explore the role of endogenous humoral substances in morphine dependence, we have studied the actions on several withdrawal effects in rats made dependent by continuous exposure to morphine, of atropine, PCPA and indomethacin, which blocks the production of prostaglandins¹²⁻¹⁴.

Rapidly Induced Physical Dependence

Dependence was induced in male albino Wistar rats (100–150 g) by a single subcutaneous injection of 150 mg/10 ml. of kg of a sustained release preparation of morphine. This was prepared by suspending 150 mg of morphine base in 0.75 ml. of an emulsifying agent, mannide monooleate ('Arlacel A'), and 4.25 ml. of light liquid paraffin. This oily phase was emulsified with 5 ml. of 0.9% w/v NaCl in water. Twenty-four hours after injection of sustained-release morphine, the rats were challenged with naloxone. Withdrawal effects occurring up to 30 min after challenge were recorded by observers who did not know what treatment each animal had received. For jumping¹⁵, the animals that jumped more times than the median value for all rats in that particular comparison were scored as positive, because of the long tail at the upper end of the frequency distribution curve. For other responses, animals that responded once or more were taken as positive.

The control experiments with ninety-two rats, summarized in Table 1, show that there was little difference between the incidence of abstinence effects precipitated by a subcutaneous dose of 0.25, 0.5 or 1.0 mg/kg of naloxone. Table 1 also shows that rats receiving the sustained-release preparation of morphine developed in 24 h a high degree of dependence, indicated by the precipitation with a small dose of naloxone of a significantly higher incidence of jumping, teeth chattering, irritability to touch or handling, diarrhoea, chewing, ptosis and body-shakes than in the corresponding controls. Two other effects—head-shakes and paw tremor—were also seen in a higher proportion of rats treated with morphine, but the incidence of these effects just failed to reach significance at the 5% level.

To confirm that the effects in Table 1 were signs of morphine abstinence, twenty-four rats were given 150 mg/kg subcutaneously of the sustained-release preparation of morphine, and, after 23.5 h, twelve animals received subcutaneously 40 mg/kg heroin as hydrochloride, and twelve

received vehicle alone (0.9% w/v NaCl in water). Twenty-four hours after morphine injection, all rats were challenged with naloxone as hydrochloride, 1.0 mg/kg subcutaneously. Heroin completely blocked jumping, irritability to touch, diarrhoea, chewing, body-shakes and paw tremors and significantly ($P<0.005$) reduced teeth chattering, irritability to handling and ptosis (Table 2).

Table 2 Blockade by Heroin, given before Naloxone Challenge, of Morphine Abstinence Signs in Rats

Abstinence sign	No. of rats positive		Significance (<i>P</i> value)
	Saline	Heroin	
Jumping	8	0	<0.001
Teeth chattering	10	1	<0.001
Irritable to touch	5	0	<0.02
Irritable to handling	10	1	<0.001
Diarrhoea	12	0	<0.001
Chewing	12	0	<0.001
Ptosis	11	4	<0.005
Body-shakes	2	0	=0.24
Head-shakes	5	1	=0.08
Paw tremor	3	0	=0.11

Twenty-four rats received subcutaneously 150 mg/kg of a sustained-release preparation of morphine (see Table 1). After 23.5 h twelve animals received subcutaneously 40 mg/kg of heroin, and twelve saline; 0.5 h later, all rats received subcutaneously 1.0 mg/kg of naloxone.

To compare this effect of heroin with that of giving it before the induction of morphine dependence, twenty-eight rats were given subcutaneously 40 mg/kg of heroin and twenty controls were given vehicle alone. Thirty minutes later the sustained-release preparation of morphine was injected into all animals. Twenty-four hours after morphine injection, six heroin-treated rats had died. The remaining test and the control rats were then challenged subcutaneously with 0.125 or 0.25 mg/kg of naloxone. Fifteen of the twenty-two animals that had received heroin jumped a total of 188 times, whereas seven of the twenty controls jumped a total of fifty-one times. The increased incidence of jumping after giving heroin before morphine induction was significant ($P<0.04$). Differences between the incidence of other withdrawal effects in the two groups of rats were not significant.

Modifying drug—atropine sulphate, PCPA methyl ester hydrochloride or indomethacin—was given in a single dose 48, 24, 1 or 0.5 h before the sustained-release preparation of morphine or 23 or 23.5 h after the morphine (that is 1 or

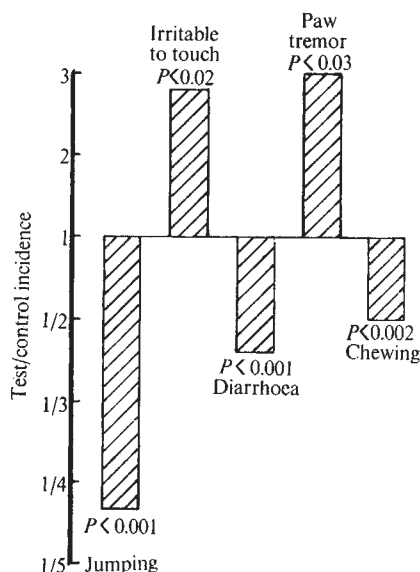


Fig. 1 Action of atropine on morphine abstinence in the rat as affected by the particular withdrawal effect observed. Abstinence was precipitated by naloxone (1.0 mg/kg subcutaneously) 24 h after injection of a sustained-release suspension of morphine (150 mg/kg subcutaneously). A subcutaneous dose of 40 mg/kg of atropine or of 0.9% NaCl in water was given 0.5 h before naloxone. There were thirty-two test animals and thirty-two controls. The test/control incidence is the ratio of the incidence of the abstinence effect in test and control groups. Atropine given before naloxone did not significantly change the incidence of the other withdrawal effects in Table 1. Other details as in Table 1.

0.5 h before challenge with naloxone hydrochloride). Doses are expressed as active acid or base. At the same time as modifying drug was administered to the test group of rats, drug vehicle (0.9% w/v NaCl or 20% w/v gum acacia in water) was given to a control group of comparable number. Both groups were challenged with the same subcutaneous dose of naloxone, which on different occasions was 0.25, 0.5 or 1.0 mg/kg. Results were expressed graphically as the ratio of the incidence of withdrawal effects in the test group receiving modifying drug to that in the corresponding control group receiving vehicle without modifying drug (the test/control incidence).

Factors determining Actions of Modifying Drugs

When atropine was administered 0.5 h before naloxone, it decreased the incidence at withdrawal of jumping, diarrhoea and chewing, whereas it increased that of irritability to touch and paw tremor and did not significantly change the incidence of other withdrawal effects (Fig. 1). These experiments, with sixty-four rats, showed that the intensity or direction of action of atropine on the abstinence syndrome can be determined by the particular effect observed.

When atropine was given shortly before the sustained-release preparation of morphine, the incidence of withdrawal effects 24 h later differed from that when atropine was given just before naloxone. Administered before dependence on morphine was induced, atropine did not significantly modify most abstinence effects, but it increased jumping and head-shakes and lessened irritability to handling (Fig. 2). The effects on jumping and irritability were in the opposite direction to those obtained when atropine was given just before withdrawal. These experiments, using a further fifty-five rats, showed that the intensity or direction of the action of atropine on some abstinence effects may also be determined by the time when it is administered in the course of dependence induction and withdrawal.

Fig. 3 illustrates the results of experiments with 153 rats

in which 86 or 172 mg/kg of PCPA or vehicle was administered intraperitoneally 24 h before morphine or 1 h before naloxone. As with atropine (Fig. 2), PCPA given before morphine increased jumping; whereas, given before naloxone, it had the opposite effect (Fig. 3). Also as with atropine (Figs. 1 and 2) PCPA, given before morphine, did not change the incidence of withdrawal diarrhoea, but given before naloxone, PCPA reduced this. In some ways, however, the pattern in which PCPA modified the abstinence syndrome differed from that of atropine. For example, PCPA, given before morphine, lessened head-shakes, whereas atropine increased them. In an experiment involving twelve test and twelve control rats, PCPA was given 48 h before morphine, but no abstinence effect was significantly modified.

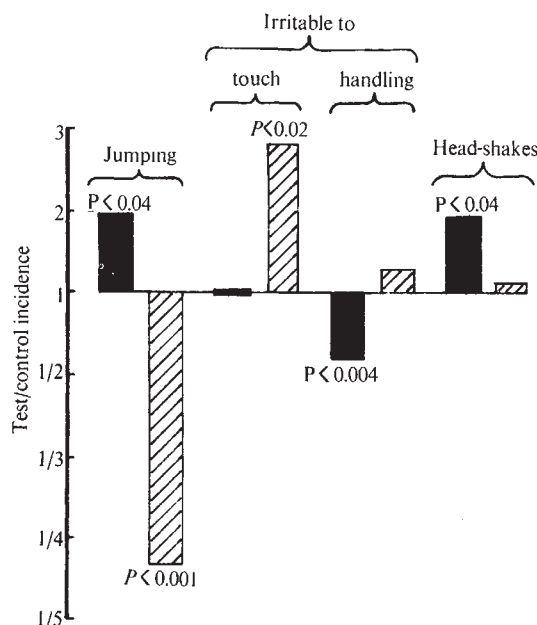


Fig. 2 Action of atropine on morphine abstinence in the rat as affected by the time of its administration in the course of dependence. A subcutaneous dose of 40 mg/kg of atropine or of 0.9% NaCl in water was given either (a) 0.5 or 1.0 h before induction of dependence with a sustained-release preparation of morphine (black columns); or (b) 0.5 h before withdrawal by challenge with 0.5 or 1.0 mg/kg subcutaneously of naloxone (hatched columns). There were fifty-nine test animals and sixty controls. Atropine given before morphine did not significantly change the incidence of the other withdrawal effects in Table 1. Other details as in Table 1 or Fig. 1.

Fig. 4 illustrates experiments in a total of 120 rats in which 25 mg/kg of indomethacin or vehicle was given orally either 1 h before morphine or 1 h before naloxone. Indomethacin did not change the incidence of several withdrawal effects, but modified that of irritability to touch, diarrhoea and chewing. Given before morphine, indomethacin lessened the incidence of irritability; but, given before naloxone, indomethacin increased it. Whereas atropine or PCPA lessened diarrhoea only when given shortly before naloxone (Figs. 1 and 3), indomethacin did so only when given before morphine (Fig. 4). Treatment with indomethacin at other oral doses (6.25 or 12.5 mg/kg) or by the subcutaneous route (25 or 50 mg/kg) had a comparable effect on withdrawal diarrhoea.

Discussion and Conclusions

A rapid test for the action of drugs on physical dependence on morphine has been developed in the rat. In this test, dependence is induced with a single subcutaneous dose of 150 mg/kg of morphine in a sustained-release suspension

and, 24 h later, abstinence is precipitated with naloxone, 0.125–1.0 mg/kg subcutaneously.

Tests of how atropine, PCPA or indomethacin modified the incidence of withdrawal effects led to the same principal conclusions. The intensity or direction of action of the modifying drug may be determined by (1) the particular withdrawal effect studied and (2) the time in the course of dependence when the modifying drug is given.

Atropine, given shortly before withdrawal, acted in opposite directions on jumping, diarrhoea and chewing, on the one hand, and on irritability and paw tremor on the other hand. This helps to resolve the seemingly conflicting results of Grumbach¹ and Fuentes³. That PCPA had the opposite effect on withdrawal jumping according to when it was given may help to resolve conflicting results previously reported for this drug^{2,3,6–9}.

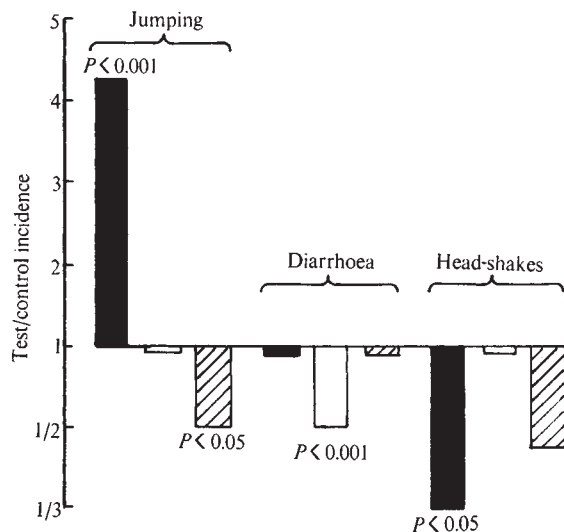


Fig. 3 Actions of *para*-chlorophenylalanine (PCPA) on morphine abstinence in the rat as affected by the particular withdrawal effect observed and the time of administration of PCPA in the course of dependence. An intraperitoneal dose of PCPA or of 0.9% NaCl in water was given either 24 h before the induction of dependence with a sustained-release preparation of morphine or 1 h before withdrawal by challenge with naloxone (0.25, 0.5 or 1.0 mg/kg subcutaneously). Black columns, 172 mg/kg of PCPA 24 h before morphine; white columns, 172 mg/kg of PCPA 1 h before naloxone; hatched columns, 86 mg/kg of PCPA 1 h before naloxone. There were seventy-five test animals and seventy-eight controls. PCPA did not significantly change the incidence of the other withdrawal effects in Table 1. Other details as in Table 1 or Fig. 1.

Where the state of dependence is measured by the signs of abstinence, the extent to which a modifying drug affects the induction of dependence or the expression of abstinence is hard to apportion. We have tried to overcome this by giving the same dose of modifying drug before induction of dependence or before precipitation of abstinence and we have supposed that drug given before morphine mainly acted on induction.

Large doses of atropine and indomethacin were used to ensure their acting during most of the induction period. At the doses used, atropine, PCPA and indomethacin may each be expected to have several effects, of which the most potent would probably be to deprive receptors respectively of endogenous acetylcholine, 5-hydroxytryptamine and prostaglandins. That each deprivatory drug affected the intensity of several morphine abstinence signs argues that each of these endogenous substances participates in aspects of morphine dependence and/or withdrawal. That a single modifying drug, whether given before induction or before withdrawal, acted in opposite directions on different abstinence effects argues that morphine dependence is "multipartite", each part embracing the induction and/or expression of one or a group of abstinence signs.

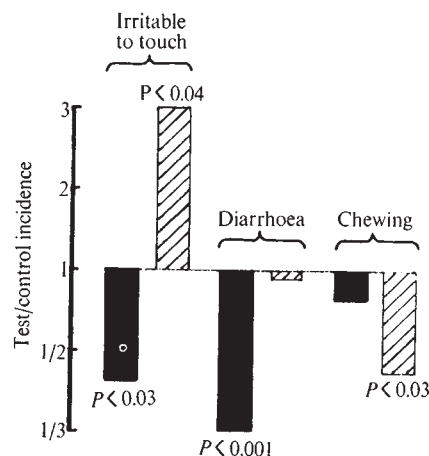


Fig. 4 Action of indomethacin on morphine abstinence in the rat, as affected by the particular withdrawal effect observed and the time of administration of indomethacin in the course of dependence. An oral dose of indomethacin (25 mg/kg) suspended in 20% gum acacia in water, or of aqueous gum acacia without indomethacin, was given either 1 h before induction of dependence with a sustained-release preparation of morphine or 1 h before withdrawal by challenge with naloxone (0.25, 0.5 or 1.0 mg/kg subcutaneously). Black columns, indomethacin given before morphine; hatched columns, indomethacin given before naloxone. The number of test animals was sixty-one and of controls was fifty-nine. Indomethacin did not significantly change the incidence of the other withdrawal effects in Table 1. Other details as in Table 1 or Fig. 1.

A change in the direction of action on morphine withdrawal effects is seen with drugs that are pharmacologically related to morphine, being cross-dependent with or antagonists of it. Thus, when heroin was given before morphine, it intensified subsequent withdrawal jumping, whereas, given before naloxone, it lessened all abstinence signs. Again, when a morphine antagonist acts during the induction of morphine dependence, it lessens subsequent withdrawal effects¹⁶; whereas, when it acts during withdrawal, it intensifies these. That the direction of action of atropine, PCPA or indomethacin on at least one abstinence sign changed with the time in the course of dependence when the drug was administered argues that the role in some parts of morphine dependence and/or withdrawal of acetylcholine, 5-hydroxytryptamine and prostaglandin(s) is intimate.

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LETTERS TO NATURE

PHYSICAL SCIENCES

Effect of Sunrise on the Meteor Region

PREVIOUS observations at Sheffield of radio echoes from Quadrantid meteors have shown a marked increase in the numbers of echoes with enhanced duration (>2 s) some tens of minutes after sunrise on the echoing layer (unpublished work of T. R. Kaiser, T. F. Nicholson and L. M. G. Poole). During the 1972 Quadrantids this increase occurred at 0830 UT, about 60 min after layer sunrise at a time when the solar zenith angle at the layer was 91° .

The specular reflexion property of meteors together with the narrow height interval in which ionization occurs (85 to 100 km) restrict echo reflexion points, from a discrete meteor radiant at a particular time, to an "echo line"¹. From a knowledge of the zenith angle and the azimuth of the radiant the position of the echo line on the aerial sensitivity contours can be calculated as a function of time. It is possible for meteor trains, which at their formation are not observed because the echo line does not intersect the aerial beam, later to produce a radio echo by virtue of wind distortion. These echoes can only be observed if the lifetime of the meteor train is longer than the time for distortion to occur.

Radio echoes from meteor trains are of two distinct types depending on whether the incident wave penetrates the train or not. The train is either underdense (electron line density, $\alpha < 2.4 \times 10^{14}$ electrons m^{-1}) or overdense ($\alpha > 2.4 \times 10^{14}$ electrons m^{-1}) and this leads to decaying or persistent echoes respectively. In the absence of electron loss or train distortion the duration of an overdense meteor train, T_0 , is given by

$$T_0 \propto \alpha \rho$$

where ρ is the atmospheric density at the reflexion point. The ionization profile of the meteor train has a maximum line density, α_{max} , proportional to the meteoroid mass, m , and this maximum occurs at an atmospheric density, ρ_1 , given by

$$\rho_1 \propto m^{1/3}$$

α_{max} and ρ_1 depend on the zenith angle of the meteor path, χ , and the meteor geocentric velocity². We related the duration of persistent echoes to the mean height of the echoing region on the meteor train by considering an influx of Quadrantid meteors (mass distribution exponent $s=1.64$, geocentric velocity 41.0 km s^{-1} , $\chi \simeq 6^\circ$ (ref. 3)) assuming that radio reflexion from a series of trains will be randomly distributed along the ionization profile. The profile used⁴ corresponded to the situation where ionization begins abruptly at an atmospheric density ρ_2 where $\rho_2/\rho_1=0.33$. Thus at 16.7 MHz an echo of duration 2 s will originate from a meteor train having $\alpha=6.3 \times 10^{14} \text{ m}^{-1}$, radio magnitude, $M_R=+3$ and having a most probable reflexion height of 94 km.

The Quadrantid shower has a radiant⁵ with a right ascension 230.5° and declination $+47^\circ$ and transits at 0839 UT. The stream is narrow, the activity of the shower usually being restricted to one day³. During January 3–5, 1972, we observed the shower with a northward directed aerial which provided time symmetry about radiant transit and sunrise. This removed the problems associated with the westward aerial previously used which, due to the small zenith angle of the Quadrantid radiant near transit, caused a sharp decrease in echo rate (due to geometrical considerations) and a change

in the range of the echoes at this time⁵. We observed at the Edgemount Research Station (lat. $53^\circ 26' \text{ N}$, long. $1^\circ 35' \text{ W}$) near Sheffield between 0530 and 1130 UT each day using a 16.67 MHz (wavelength, λ , 18.0 m) pulsed radar (p.r.f. 50 s^{-1} , pulse length 100 μs) which radiated 10 kW peak power from a horizontal rhombic aerial of leg length 1.4λ , height 0.8λ , half angle at minor axis 48° . The parameters of the meteor echo that were measured were the time of occurrence, range, duration and amplitude–time profile. The region of the atmosphere observed was at a ground distance of approximately 350 km to the north of Sheffield and was centred on a height of 90 km. On January 5 the time of sunrise for the echoing layer was 0726 UT. The solar zenith angle was 99.2° at 0726 and had decreased to 90.0° by 0840.

Fig. 1 shows the number of underdense echoes observed during a 10 min period as a function of time. These echoes had an observed duration, T , of less than 0.5 s and could only be seen when the reflexion was specular. Underdense Quadrantid meteors can only be seen before 0700 on January 4. Fig. 2 shows the number of overdense echoes with T greater than 1.9 s ($\alpha > 6 \times 10^{14} \text{ m}^{-1}$, $M_R < 3.1$) as a function of time. The specular overdense echoes peak at 0620 and then disappear as the echo line moves out of the beam. There is another period of enhanced activity which starts at 08 h 30 ± 6 min UT. This is well after layer sunrise (0726 UT) and occurs when the solar zenith angle on the layer is $91.2 \pm 0.7^\circ$. The second activity peak could not be caused by the echo line re-entering the aerial beam, because this would cause a large increase in the underdense echo rate. It can be seen from Fig. 1 that this does not occur. The narrowness of the Quadrantid stream is well demonstrated in Fig. 2. On January 3 the Earth had not reached the stream and only sporadic meteors were observed, the solar longitude, $\lambda_\odot$, at 0830 UT

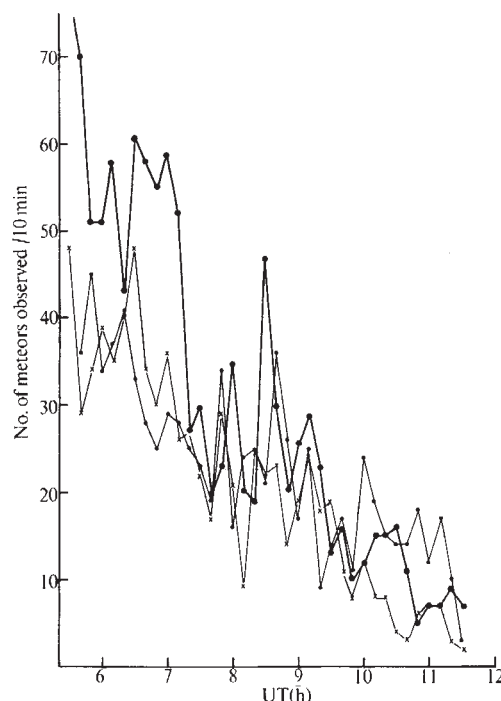


Fig. 1 The number of underdense meteors ($T < 0.5$ s) observed during a 10 min period as a function of universal time. ---, January 3; —●—, January 4; —x—, January 5.

on January 3 being 281.77° . On January 4 ($\lambda_\odot$, 282.80°) the Quadrantid shower was present and the observed overdense meteor rate had increased by approximately 340%. But on January 5 ($\lambda_\odot$, 283.81°) the Earth had completely passed through the stream and again only sporadic meteors were observed. By taking the mean sporadic influx from January 3 and 5 and subtracting this from the influx on January 4, only the Quadrantid meteors are left. These Quadrantid meteors were then separated into night-time meteors occurring before 0830 and daytime meteors occurring after. Fig. 3 shows $N(T)/N(1.9)$ (expressed as a percentage) as a function of T , where $N(T)$ is the number of meteors having a duration greater than or equal to T . Also shown is the most probable reflexion height for each echo duration. The linear portion of the data in Fig. 3, for $T \leq 8$ s, is simply a consequence of the power law of meteoroid mass distribution and the slope of this region may be related to s . Referring again to Fig. 3, notice the differing gradients of the linear portion of the plot for day and night and also the distinct knee in the curve at $T \approx 12$ s and rapid fall away for $T > 12$ s. The different initial duration distribution for $T \leq 8$ s (corresponding to heights between 91 and 94 km) must result from an atmospheric effect because only Quadrantid meteors were used in the analysis and there is no day-night change in geocentric velocity, there is negligible change (less than 0.3 km) in the ionization heights due to changes in zenith angle, and s remains constant over the observing period³.

The obvious candidates for producing a "sunrise effect" are (i) the greater meteoric ionization loss at night due to changes in the atmospheric concentrations of the minor species O, O₃ and O₂ ($^1\Delta_g$) and (ii) an increase in large scale turbulence due to atmospheric heating after sunrise which leads to an increase in wind shear and thus an increase in the number of persistent non-specular echoes.

The observed duration T of a persistent meteor may be related to the duration T_0 in the absence of ionization loss by a time constant τ such that

$$T = T_0 \exp \left(-\frac{T}{\tau} \right)$$

(This is only strictly true for an attachment electron loss mechanism but τ nevertheless is a convenient parameter representing the reduction in echo durations.) If one assumes that the effect of large-scale wind shears are always negligible and that the daytime $T \leq 8$ s line is the "no loss" case, then the night-time $T \leq 8$ s line can be explained by a constant value of τ ,

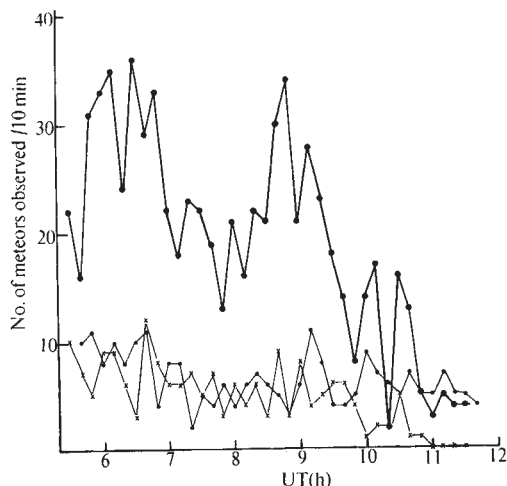


Fig. 2 The number of overdense meteors ($T > 1.9$ s, $\alpha > 6 \times 10^{14} \text{ m}^{-2}$, $M_R < 3.1$) observed during a 10 min period as a function of universal time. $\cdots$, January 3; $\bullet$ — $\bullet$, January 4; $\times$ — $\times$, January 5.

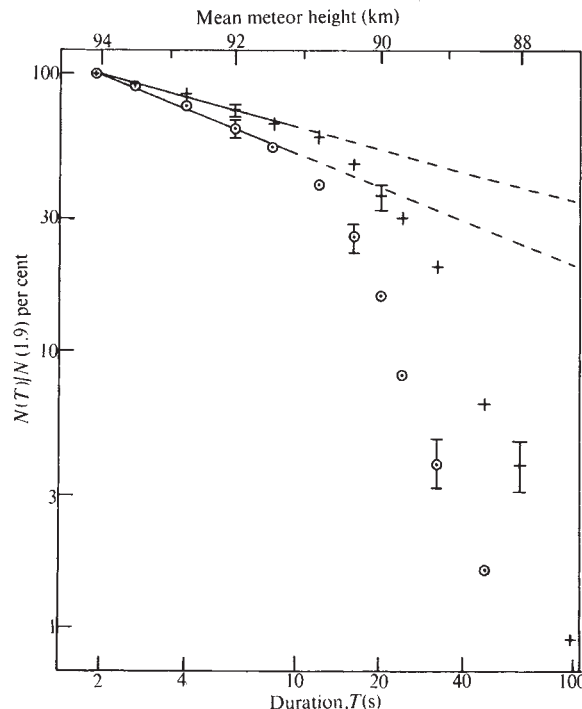


Fig. 3 The integrated duration distribution of overdense Quadrantid meteors, $N(T)/N(1.9)\%$, as a function of T . $\circ$, Night-time meteors; $+$, daytime meteors.

of the order of 15 s at heights from 91 to 94 km. The only atmospheric species showing significant diurnal variation in this height region are O₃ and O₂ ($^1\Delta_g$). The increased night-time O₃ concentration will result in faster associative attachment and hence ionization loss. But accepted values of O₃ number densities are neither sufficiently high nor show a great enough diurnal variation to produce a loss constant as fast as 15 s. We therefore suggest that a post sunrise increase in wind shears, which will be present over the whole meteor region up to the turbopause (105 km), is responsible for the change in the observed echo duration distribution for $T \leq 8$ s. On the other hand the drastic reduction in the number of echoes with $T > 12$ s in both daytime and night-time observations is definitely due to a loss of ionization. The difference between the two curves indicates a real day-night change in the electron loss processes. From Fig. 3 we can deduce that at 85 km τ changes from a night-time value of 6.8 s to a daytime value of 10.1 s. We can also estimate the variation of τ with height and find that it decreases with altitude a little more slowly than the atmospheric density.

W. J. Baggaley (unpublished work) has shown that the observed diurnal changes in overdense echo distributions for $T > 12$ s (height less than 91 km) can be adequately explained in terms of the chemistry of the region. Below 90 km meteoric ionization loss is controlled by both electron-ion recombination and by attachment (associative attachment with O₃ and three body attachment to O₂). Night-time loss is more rapid because of changes in the O₃ concentration, changes in O and O₂ ($^1\Delta_g$) playing a secondary role. T. F. Nicholson (private communication) has suggested an additional process in which rapid photodetachment of negative ions with large electron affinities, such as NO₃⁻, takes place during the "sunrise period". NO₃⁻ is thought to be produced at heights lower than ~ 85 km by reactions involving O₂⁻ (this coming from electron attachment).

These observations show the value of using Quadrantid radio meteors—low s giving proportionally more large meteoroids and hence more long duration echoes; constant geocentric velocity, zenith angle and s over the observing period and a transit close to local sunrise—to provide informa-

tion about the aeronomy of the atmospheric region between 80 and 95 km.

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Lava Filled Craters and the Thermal History of the Lunar Surface

THE concept of a "cooled crust" is useful in studies of the thermal history of a planetary surface originating from a differentiated liquid layer of considerable thickness. The cooled crust is defined as the solid outer layer of a planetary surface increasing in temperature with depth, the lower boundary of which, at a depth Z_0 , is at a temperature T_0 corresponding to the zero-pressure melting point of the planetary material. The physical state of the material below the boundary Z_0 will depend on the thermal gradient dT/dZ and the elevation of the melting point with increasing depth due to lithostatic pressure which we indicate by dM/dZ . I assume material of basaltic composition under lunar conditions¹.

If the temperature gradient in the vicinity of Z_0 is appreciably greater than the rate of elevation of the melting point due to increasing depth, $dT/dZ > dM/dZ$, a layer of melted material will result. This case (which I call hypothesis I (ref. 2)) corresponds to the lunar model suggested by Wood *et al.*^{3,4} where a crust of solid anorthosite floats on a magma of dense basalt.

In the second case (hypothesis II) the material below Z_0 is at a temperature greater than T_0 but is held in the solid state by lithostatic pressure ($dT/dZ \leq dM/dZ$). In both these cases a meteorite penetrating well below Z_0 will result in a lava filled crater. For hypothesis I the already liquid lava of the magma will rise to occupy the depression caused by the impact, and in hypothesis II the release of load will result in partial or complete melting of high temperature material held in the solid state by lithostatic pressure.

As cooling proceeds, however, the thickness of the cooled crust will continually increase and lava filled craters of hypothesis I and hypothesis II type will in general no longer be produced. Referring now to the lunar surface, these considerations account for such ancient lava filled craters as Gagarin, Korolev and Pasteur but fail to explain features like Archimedes and Plato which also have flat lava floors but are clearly of much more recent age. I have suggested that these and other features near circular maria could be explained as a consequence of lava released by the very large impacts which produced the maria themselves², regarding them as hypothesis II events of unusual magnitude. Although it still seems possible that a substantial proportion of the maria filling may have been due to this cause, recent studies of Apollo and Orbiter photographs indicate that other relatively recent features on the lunar surface could only be ascribed to this cause with difficulty. One of these is Mare Smythii (diameter ~425 km), a somewhat ill-defined circular mare which may

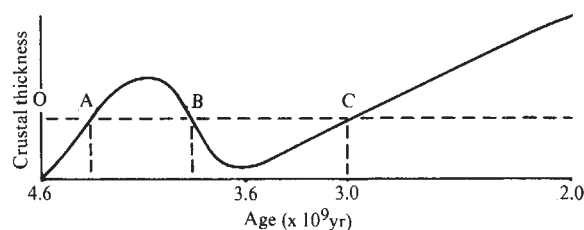


Fig. 1 Qualitative representation of assumed variation in thickness of the cooled crust of the Moon during successive periods of lunar surface heating.

have been due originally to impact, the presence of drowned craters near its centre indicating that its present surface has had a different origin, presumably a lava flow of considerably more recent age. Another example is Tsiolkovsky, 175 km diameter, also lava filled and of relatively recent origin, considered due to an impact of insufficient energy to pierce a thick crust.

In order to explain these features it seems necessary to invoke a second heating of the Moon, a possibility already suggested by the age determinations of lunar materials. The situation is illustrated in Fig. 1 where the positions of the minima of the curve correspond to the estimates of ages of lunar materials, which cluster around 4.6 and 3.6×10^9 yr (refs. 5-9). The horizontal dotted line corresponds to a crustal thickness too great to be pierced, except, possibly, by exceptionally large impacts.

Here, I assume that there are two periods of lunar surface heating and therefore two periods during which the cooled crust is thin enough to make lava filled craters possible. These two periods are OA and BC, and although the time lapse BC may be longer than OA, the impact frequency is probably much greater during the early period of lunar history represented by OA. No doubt craters in the very early stages of crustal formation would be destroyed by later events so that only large craters near the limit A would be likely to survive. The thinning of the cooled crust between B and C would probably be strongly influenced by cracks and fissures and other inhomogeneities so that the lower boundary of the cooled crust, Z_0 , T_0 , could be very irregular. Although the thinning of the crust would again make hypothesis I and hypothesis II craters possible, it might be difficult to distinguish them from older normal craters infiltrated from beneath by lava intrusions. The distinction between them, an interesting geophysical problem in itself, is of only minor importance to their function as indicators of increased lunar surface temperature.

In order to provide a semi-quantitative test of the hypothesis of two periods of high surface temperature separated by a period of cooling and thickening crust, 8 lava filled craters or lava covered areas were examined using the number of small craters 2 km diameter and over, per 10^4 km², as an indication of relative age. Table 1 shows that the craters fall into two distinct groups, one group having counts per unit area of 67 to 113 and the other group much smaller counts, from 5 to 12. A specimen crater from each of the two groups is shown in Fig. 2. The inference is that these two groups

Table 1 Small Crater Counts: Diameter of Index Craters 2 km and over Unit Area 10,000 square km

Flat floored craters	No. of index craters per unit area
Gagarin	113
Korolev	70
Deslandres	76
Pasteur	67
Archimedes	12
Plato	8
Tsiolkovsky	6
Mare Smythii	9

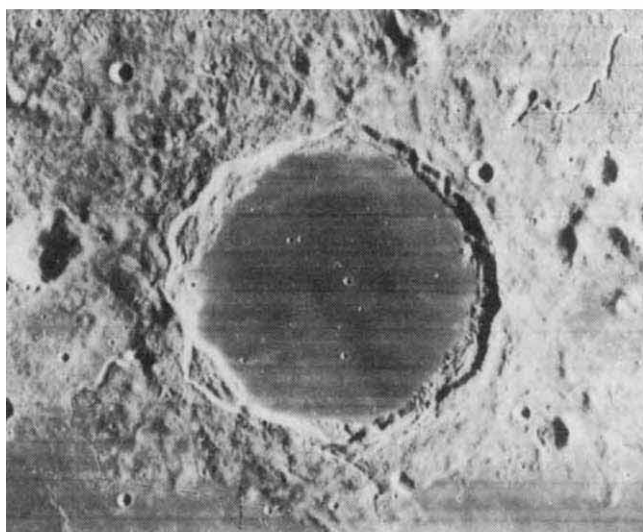
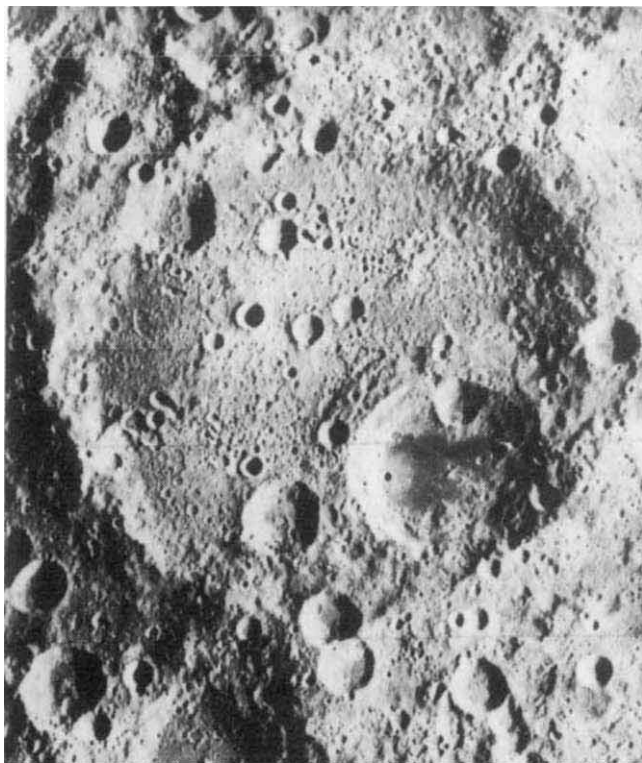


Fig. 2 The crater Gagarin (above), 250 km diameter, is considered to be associated with the first period of lunar surface heating. The crater Plato (below), 101 km diameter, clearly of more recent age, is assigned to the second period of lunar heating. Both craters are tentatively interpreted as Hypothesis I events, with the impacting meteorite piercing a floating crust.

correspond to events separated by a long interval of time, and the data are thus consistent with the general history presented in Fig. 1. A further programme of crater counts is now in progress to determine whether the hypothesis of two separate periods of lunar surface heating can be confirmed by a more extended sample.

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Non-velocity Redshifts and Photon-Photon Interactions

RECENT data on galactic redshift anomalies have led Hoyle and Narlikar¹, for example, to propose a qualitative explanation in terms of spatio-temporal variation of proper mass. The consequences of hypotheses such as this are evidently worthy of study, but there are other redshift anomalies, which should also be considered in this context. A possible alternative explanation is in terms of an inelastic photon-photon interaction; this has the advantages of being more inclusive, more quantitative (at least in order of magnitude) and immediately testable in the laboratory.

We shall show that such an explanation has several important features. First, it accounts also for the unresolved discrepancy in the solar redshift at the limb; second, the average energy-loss parameter derived from the solar data can explain the galactic redshift anomalies in terms of variations in source temperature; third, such a source-dependent effect can explain an anomaly at present unresolved in the distribution of pulsars at high galactic latitudes; and, fourth, the sensitivity of currently available apparatus allows an immediate laboratory test of the existence of this interaction, at least for incident gamma rays.

Our basic idea is not new, and resembles that of Born² and Finlay-Freundlich³, who proposed a law of the form

$$z = \delta v/v = -AT^4l$$

to account for anomalies in the redshifts of B and O-class stars, and also for the Hubble redshift. This proposal has, however, encountered severe quantitative difficulties (discussed by Melvin⁴, and also by Cayrel (private communication) who has found that a T^4 law gives impossible shifts for blue supergiants).

Among possible photon-photon interactions there is the Heisenberg-Euler process, but this is too weak to be important. The most likely interaction, which does not upset established results of quantum electrodynamics, seems to be that derived from a Lagrangian term

$$\Lambda(A_\mu^* A^\mu A_\alpha A^\alpha + \text{conj.})$$

associated with the graph of Fig. 1, in which the energy lost goes into a soft photon pair.

Such a process implies the existence of a photon rest frame and thus of a photon rest mass (necessarily $< 10^{-48}$ g on experimental grounds⁵). We shall not discuss the consequences of such an implication in this communication which, as will become clear, is essentially phenomenological.

To obtain the Doppler form, it must be assumed that the fractional energy loss per cm per unit density of scatterers of frequency ν_2 ($\Delta(\nu_2)$) is independent of the incident frequency ν_1 .

It then follows that

$$z = -\delta v_1/v_1 = \Delta(v_2)n_2\delta l \quad (1)$$

for a trajectory of length δl through a density of scattering photons n_2 . For a black-body background, where $\Sigma n_2 \approx 30 T^3$, equation (1) may be written

$$Z = A(T) T^3 \delta l \quad (2)$$

(The different exponent, T^3 in place of T^4 , allows equation (2) to meet successfully the objections to the Finlay-Freundlich relation cited above.)

At least for photon energies much below the threshold for electron-positron pair production (as in all the cases considered here), significant variations in $A(T)$ with temperature are unlikely and we shall test the usefulness of equation (2) when A is taken to be constant.

We first consider the solar redshift—by several orders of magnitude the most accurately determined. It is known (refs. 6, 7 and J. C. P., A. P. R. and J. P. V., to be published) that observations near the limb give, on extrapolation to the limb itself, a redshift, z , significantly exceeding the Einstein gravitational redshift; Δz is in the range $2 \times 10^{-7} < \Delta z < 10^{-6}$. Although discrepancies elsewhere on the solar surfaces may be attributed to correction factors sometimes poorly known (effects of Lindholm, Spitzer, radial Doppler, optical rugosity and geometrical rugosity), these effects are all zero at the limb⁶ and cannot be invoked.

We now assume that equation (2) accounts for this residual shift by an inelastic interaction of the photon with the solar radiation field. A point source approximation to this field⁴ readily yields

$$\begin{aligned} \Delta z &= A T_{\odot}^3 R_{\odot} \int_0^{\pi/2} \text{cosec}^{1/2} \theta d\theta \\ &= 2.6 A T_{\odot}^3 R_{\odot} \end{aligned}$$

With $T_{\odot} = 5,800$ K, $R_{\odot} = 7.0 \times 10^{10}$ cm

we find $0.6 \times 10^{-29} < A < 3 \times 10^{-29}$ degree⁻³ cm⁻¹ (3)

Can the other redshift anomalies also be accounted for, at least in order of magnitude, by the same value of A ? The variations are all between linked galaxies which are young and we therefore consider the results given by equation (2) for HI (non-ionized) and HII (ionized) clouds.

Taking representative temperatures $T_I \approx 100$ K, $T_{II} \approx 1,000$ K, equation (2) gives the ratio

$$z_{II}/z_I = 1,000(R_{II}/R_I) \quad (4)$$

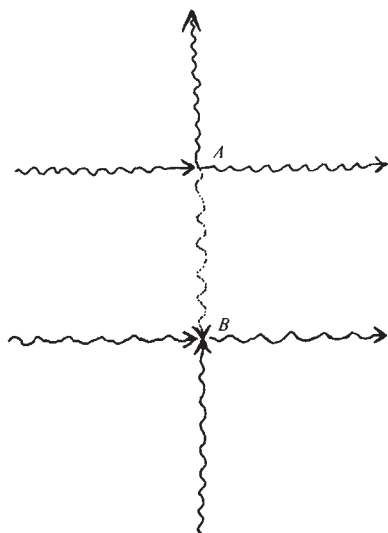


Fig. 1 Photon-photon inelastic scattering. A virtual photon is exchanged between vertices A and B.

The observational problem is to account for variations in z by factors up to ~ 10 . Equation (4) shows that, given intermediate formations of partial ionization, variations of this order can indeed be expected, even if $R_{II} \ll R_I$.

To verify that the actual values of z following from equation (4) are reasonable, values for the radii, which are uncertain quantities, would be needed; but assuming $R_I = 10^{21}$ cm (330 pc), for example, equations (2) and (3) yield values of z_I in the range 0.006 to 0.03, an acceptable result.

The other anomaly—relative velocities apparently exceeding c —would, of course, arise from a wrong interpretation of a source-dependent z as an indication of velocity and hence of distance.

There is also an unresolved difficulty in the distribution of pulsars. Of the fifty-three pulsars known on July 6, 1971 (A. Hewish, private communication), no less than ten lying in the cone of galactic latitude greater than 30° have dispersion measures (DM) > 10 . (The DM (electrons cm⁻³ pc) is usually interpreted as the result of scattering from interstellar electrons, so it can be used as a distance measure⁸.)

Assuming an average electron density of 0.03 cm⁻³, and a galactic disk thickness of 150 pc in the neighbourhood of the Sun, it is easy to show that stars in this cone cannot be distant enough to give a DM > 2.6 if they lie within the galactic disk. The usual interpretation of the DM thus forces the supposition either that there are large increases in electron density within such a cone, or that the pulsars in question have an anomalously large population density outside the disk of the Galaxy. (The latter could be a consequence of observational selection, but this is a difficult proposition to substantiate.)

The whole difficulty disappears, however, if a source-dependent effect is responsible for the anomalous surplus in the DM. Although such an effect cannot at present be calculated quantitatively in the framework of our hypothesis, because it depends on a slowing down of the photon as a result of the interaction (that is, an effective real refractive index) rather than the energy loss treated here, its magnitude is probably non-trivial as indicated by the very high surface temperatures $\sim 10^6$ K and consequent high density of target photons assigned to pulsars in neutron star models⁹.

The "cosmological redshift" can hardly be considered an "anomaly", but it is interesting to consider the redshift due to a universal background of temperature T . (We note, however, that even if an inelastic process were established for the effects discussed above, it would need to be applied with caution to a temperature in so different a range, that is, to the scattering of photons so much less energetic.) From the law $v = Hl$ and equation (2), it would follow that $T^3 = H/cA$. If H is in the range 50 to 100 km s⁻¹ Mpc⁻¹, and A is as given by equation (3), it follows that

$$1.2 \text{ K} < T < 2.7 \text{ K}$$

a range quite close to the currently favoured value of ~ 2.75 K.

The above hypothesis evidently requires more detailed formulation, but it has the advantage of confronting anomalies other than the galactic redshifts, and of being amenable to a laboratory test. The last of these possibilities would require a repetition, in different circumstances, of the classic experiment of Pound and Rebka¹⁰ which confirmed the gravitational Einstein redshift for a photon emitted at a height d . A fractional energy loss Δ_G was determined through the Mössbauer effect, where

$$\Delta_G \approx gd/c^2 \quad (5)$$

We propose a similar experiment, with the gamma-ray photon (frequency ν_1) following a horizontal path traversed also by a coincident laser pulse or pulses. For a pulse of total energy W cross section a , and optical frequency ν_2 , the energy loss due to any inelastic collisions is given by equation (1) as

$$\Delta_I \approx (W/h\nu_2 a) \Delta(\nu_2)$$

Comparing this with equation (5), the condition for detectability (that is, $\Delta_1 < \Delta_G$) turns out to be

$$\Delta(v_2) > g d a h v_2 / c^2 W$$

or, on inserting the values $d = 25 \text{ m}$, $a = 0.1 \text{ cm}^2$, $\lambda_2 = 5000 \text{ \AA}$, $W = 10^8 \text{ erg}$,

$$\Delta(v_2) > 10^{-35}$$

Now this detectable value is less by a factor of 10^5 than that given by the solar data ($\Delta(v_2) \simeq A/30 \simeq 10^{-30}$). Although in the latter case optical photons are scattered rather than gamma rays—so that the supposition of an inelastic interaction could still be maintained in the face of a negative result by abandoning the dependence on v_1 assumed for the energy loss—the Doppler-shift form could evidently no longer be derived, thus making the experiment crucial for a large number of hypotheses involving inelastic photon interactions, including the one outlined above.

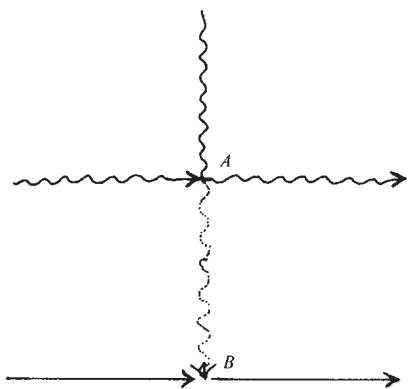


Fig. 2 Photon-electron inelastic scattering. A virtual photon is exchanged between vertices A and B.

A further possible experimental check is suggested by the observations of Laporte (mentioned by ter Haar in reference 11). He found that the spectra of hot gases behind fast shocks show spectrum lines which are shifted to the red as well as broadened. This is to be expected if virtual photons are exchanged between incident photons and charged material—a process following from the Lagrangian term above, and the graph of Fig. 2. Further experimental data would be of obvious interest.

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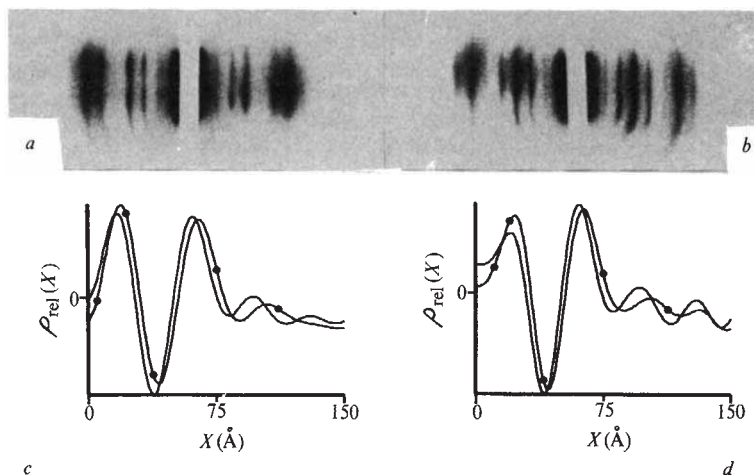
Lamellar Structure of Bleached and Unbleached Rod Photoreceptor Membranes

WITHIN the past 3 yr, X-ray diffraction data from intact retinal rod outer segments have been obtained in several laboratories with sufficient resolution to permit analyses of the principal electron density fluctuations associated with the disk membranes¹⁻⁶. In 1969, Gras and Worthington¹ published diffraction patterns from fresh rod outer segments (*Rana catesbeiana*), showing the first 8 orders of a 314 Å repeat. They interpreted their data in terms of a linear strip density model, which featured an asymmetric membrane with the wider high density strip adjacent to the intradisk space. They considered it reasonable to assign the photopigment molecules to this wider strip. Later that year, Blaurock and Wilkins² published slightly higher resolution diffraction data from the rod outer segments of *Rana temporaria*, but handled their data somewhat differently. Their choice of an electron density profile was based on an initial interpretation of the Patterson map in terms of a lipid bilayer. Their phase choices were subsequently found to agree with swelling and shrinking studies using Ringer solutions of various tonicities. Their membrane profile appeared largely symmetric, in contrast to the linear strip model. The membrane profiles derived by both groups featured lipid bilayer regions, but the symmetry of the membrane, and hence the distribution of rhodopsin in the membrane, was in dispute.

At the same time X-ray diffraction studies were in progress in my laboratory to determine the disk membrane profile, and the location of rhodopsin within this profile, in an effort to resolve the discrepancies in interpretation. It was first hoped to phase the reflexions by sampling the continuous transform of the disk at additional points with the help of osmotic swelling and shrinking experiments, and then applying the Shannon sampling theorem to the results⁷. Strips of dark-adapted retinas (*Rana pipiens*) were dissected under dim red light, and folded to appose the outer segment layers, effectively doubling the number of diffracting units in the X-ray beam. The rod outer segments were shrunk by including various percentages of sucrose in the frog Ringer solution. Fig. 1 shows some typical diffraction patterns obtained. Orders 3 to 8 are strikingly different in the two patterns, although the unit cells differ in size by only 6 Å. The patterns are reproduced to the same scale and are directly comparable. Clearly the changes in intensities noted for orders 3 to 8 cannot be entirely accounted for by changes in the sampling frequency. There must also be a change in the continuous transform which is sampled, that is there must be a change in the structure of the disk. In practical terms, such changes might involve the membrane structure itself, and/or the distance between the two membranes constituting the disk.

Several pieces of evidence indicate that the membrane structure is essentially unaltered. The Patterson peak seen near 40 Å is interpreted here, and also by Blaurock and Wilkins², as arising principally from vectors within each membrane of the disk. The fact that this peak does not significantly change in response to increasing sucrose concentrations strongly suggests that the distribution of matter within the membrane does not change with the addition of sucrose. Such an observation is important since it is known that sucrose can preferentially destroy or render unavailable for extraction one of the photopigments of the lizard, *Gecko gecko*⁸. Several other pieces of information are pertinent: (1) the spacing and intensity changes are reversible; (2) retinas can still be bleached after soaking in Ringer solution containing up to 30% w/w sucrose⁹; (3) the sign of the birefringence does not change when sucrose is added to the medium; and

Fig. 1 *a*, X-ray diffraction pattern (1 h) from frog retina in 3% sucrose-Ringer solution (unit cell size $d=295$ Å). Integrated intensities for the first 11 orders of diffraction in arbitrary units are: 1,840, 55, 168, 460, 76, 470, 858, 128, 61, 39, 17. *b*, Diffraction pattern (1 h) from retina in 6% sucrose-Ringer ($d=289$ Å). The integrated intensities are: 1,480, 34, 256, 860, 260, 116, 817, 315, 75, 33, 21. *c*, Overlapping electron density profiles deduced from disk membrane diffraction. The relative electron density function ($\rho_{\text{rel}}(X)$) is plotted from the centre of the disk ($X=0$) to about the centre of the cytoplasmic region ($X=150$). The membrane is located approximately between $X=0$ and $X=80$. The structure factor amplitudes were derived from intensity data by applying Lorentz factor $(\sin 2\theta)^{-1}$. For low angles, this correction is $|F(h)|^2 = h \cdot I(h)$. The first 8 orders have been included. Note the membrane shift and similarity of profiles derived for the 3% (—●—) and 6% (—) sucrose-Ringer patterns. *d*, Electron density profiles for 3% (—●—) and 6% (—) sucrose-Ringer data derived from first 11 orders. The phases for both sets of data are: $\circ, \pi, \circ, \circ, \circ, \pi, \pi, \circ, \circ, \circ$. Note the reversal in relative heights of the two membrane peaks. As higher orders are included, the height of the cytoplasmic peak increases relative to the peak adjacent to the centre of the disk.



(4) the disk membranes appear unchanged in electron micrographs.

What is the evidence that a membrane shift is involved? Blaurock and Wilkins² have suggested that the peak in the vector map near 85 Å represents the cross-correlation for the membranes of the disk. If such is the case, then the position of this peak should change as the membranes of each disk move closer together or further apart. As the percentage of sucrose is increased, only the spacing of the second peak in the vector map changes significantly, indicating that the membranes of each disk are moving towards one another. This shrinking of the disk is consistent with the osmotic sensitivity of the disks noted by Clark and Branton¹⁰, DeRobertis and Lasansky¹¹, and by Dowling¹², the osmotic behaviour of isolated outer segment fragments noted by Brierley *et al.*¹³, and infiltration studies by Cohen¹⁴.

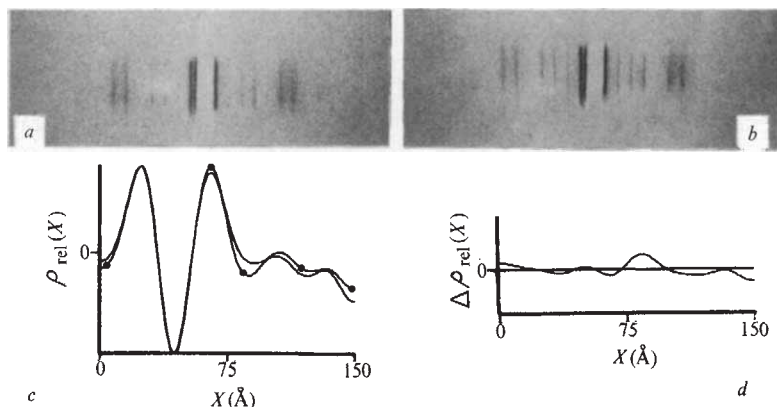
The phases for the sets of diffraction data in Fig. 1 can be determined using the following criteria: (1) the existence of a continuous interval about 60–70 Å wide near the centre of the disk whose electron density profile is identical in the correct solution for each set of intensities, and whose shift in position accounts for the shift observed in the corresponding vector maps; (2) the spacings between electron density extrema within the membrane must be the same in the correct solution for each; and (3) the correct density plot for each set of intensities should include an extended region of approximately constant density in the cytoplasmic portion of the unit cell. These criteria permit the choice of one set of phases for each set of intensities. The resulting electron density profiles are presented in Fig. 1c and d.

The density distribution in the membrane region is nearly symmetrical and is characterized by two high density peaks

spaced 40 Å apart, both equidistant from the centre of an intermediate low density trough. This profile is very similar to that deduced by Blaurock and Wilkins². It is significantly different from the strip model derived by Gras and Worthington¹. In all three studies, however, the preponderance of evidence indicates that a rather highly disordered lipid bilayer is a prominent architectural feature of the membrane, as predicted by the unit membrane theory of Robertson¹⁵. A similar phasing method has also been used by Blaurock and Wilkins¹⁶ recently, with analogous results.

The location of rhodopsin is not obvious from the electron density profile of the membrane. Blaurock and Wilkins² came to this same conclusion and suggested that rhodopsin might be equally distributed on both sides of the membrane. Gras and Worthington¹, however, assigned the photopigment molecules to only one region of their membrane strip model—the wider high density strip adjacent to the intradisk space. How might it be possible to determine where rhodopsin is located? One approach to the problem is to take advantage of the conformational changes of rhodopsin in response to bleaching, as suggested by circular dichroism and optical rotatory dispersion studies^{17–22}, absorption changes near 235 nm (refs. 23 and 24), and several other techniques^{18,25}. These data have been reviewed by Mommaerts²² and several others^{26–28}. Figs. 2a and b show the X-ray intensity distribution obtained from the same retina before and after bleaching. Small but significant changes are evident in orders 2, 3 and 4. The size of the unit cell in this particular experiment increased by only 1 Å; the position of the second Patterson peak did not change. Thus, the intensity changes primarily reflect structural changes in the disk membranes. These reflexions were scaled to the origin peak of the vector map, and phased

Fig. 2 *a*, Diffraction pattern (30 min) from an unbleached frog retina in standard Ringer solution (no sucrose). *b*, Thirty minute pattern from same retina after bleaching. Some weak reflexions from the Cu K β radiation are evident due to the thinness of nickel filter. *c*, Overlapping electron density profiles determined for bleached (—) and unbleached (—●—) disk membranes. Ten orders of diffraction have been included. The data were scaled to the origin peak of the vector map. *d*, Electron density redistribution ($\Delta\rho_{\text{rel}}(X)$) accompanying bleaching. The function is plotted as the bleached profile minus the unbleached profile; the same scale is used as in *c*. Note the predominant rearrangement near 80 Å.



by the sucrose method outlined above. The results are presented in Fig. 2c and d. Fig. 2c shows the overlapping profiles for the bleached and unbleached specimen. Three differences appear: one near the centre of the disk ($X \approx 0$), another in the cytoplasmic region ($X \approx 90$ to 150), and a third at the cytoplasmic interface of the membrane ($X \approx 80$). The first two differences represent fluctuations which vary in amplitude and sign in different experiments, are usually smaller than depicted, and are not specific for bleached membranes. The change in the density distribution near the cytoplasmic interface, however, is found consistently in bleached retinas. The differences in the profiles are more clearly presented in Fig. 2d, emphasizing the redistribution of matter near the cytoplasmic interface. Vitamin A is the only compound thought to be lost from the outer segment following bleaching²⁹, and represents an insignificant fraction of the total mass of the outer segment. Thus, the difference function plotted in Fig. 2d should add to zero when summing over all the membrane coordinates. This is not exactly true when summing over the interval (10 Å–100 Å). This discrepancy may reflect a series termination effect.

These observations provide evidence for the gross localization of a part of the rhodopsin molecule near the cytoplasmic interface. This position is not to be confused with the membrane interface adjacent to the centre of the disk, which has been referred to by Worthington³⁰ as the "cytoplasmic side". Blasie³¹ and Dewey *et al.*³² have provided evidence that rhodopsin is located in part at an aqueous interface.

If rhodopsin is embedded in only one side of the membrane, as the bleaching experiments suggest at first glance, then there seems to be some discrepancy in the electron density profile. Rhodopsin is known to constitute about 50% of the dry weight of the rod outer segment disks^{33,34}. Yet the observed profiles for the electron density, of both bleached and unbleached outer segment membranes, do not permit 50% of the electrons to be fitted on one side. There seem to be two principal possibilities: (1) that the profiles are not accurate owing to errors in the structure factor amplitudes resulting from possibly inadequate intensity correction factors; or (2) if the density profiles and the interpretation of bleaching experiments are essentially correct, then rhodopsin may be distributed equally on both sides of the membrane, but the response to bleaching may be different on each side of the membrane. The two relaxation times observed for the photodichroism of rhodopsin by Cone⁹ may be pertinent to the second point. There may also be a third possibility, namely that the density changes accompanying bleaching do not result from extensive conformational changes of the rhodopsin molecule^{35–37}. The change in electron density at the cytoplasmic interface noted on bleaching indicates that there is a redistribution of matter, that is, some of the matter in the high density peak at the cytoplasmic interface has shifted towards the cytoplasm. The two high density peaks of the membrane are still 40 Å apart, however, and equidistant from the central low density trough. These experiments have not yet indicated that there is a shift of matter into the membrane³¹.

The distribution of rhodopsin in the outer segment disk membranes is still unsettled. Studies in progress on the heavy metal labelling of the chromophore sites will be published separately.

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Transmission of Australia Au(1) Antigen by *Culex* Mosquitoes

THE prevalence of Australia Au(1) antigen in apparently healthy individuals is high in the tropics and low in temperate climates¹. Genetic factors have been incriminated¹; but Prince² suggested that insects might be vectors. This study was undertaken to find out the role of the common blood-sucking mosquitoes in Ibadan, Nigeria, in the transmission of Au(1) antigen.

Colonies of *Anopheles gambiae*, *Culex pipiens fatigans* and *Aedes aegypti* were maintained from the egg to the adult stage. Newly emerged mosquitoes were starved for 2 days and later experimentally fed with an artificial blood device using blood with and without Au(1) on the experimental and control groups. Immediately after the blood meal, some of the mosquitoes were killed with ether and snap frozen in liquid nitrogen (-70°C), and cryostat sections were taken from the gut and thorax. These were stained by immunofluorescent techniques using guinea-pig antiserum to Au(1) and FITC anti-guinea-pig gammaglobulin for the indirect and FITC rabbit anti-Au(1) for the direct method. Washing was in phosphate buffer saline (PBS), pH 7.5, for 15 min with three changes. The sections were mounted in 90% glycerol and viewed with a Leitz fluorescence microscope. The specificity of the reaction was confirmed by the absorption method.

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The mosquitoes were made to feed on restrained and narcotized chickens, given intramuscular nembutal. Soon after the feed, the chickens were killed and sections taken from the bitten areas and the liver for immunofluorescent studies. Some mosquitoes were killed 5 days and 10 days after the initial feed for a repetition of the immunofluorescent tests. The remaining mosquitoes were made to feed on adult chickens weekly. The bitten areas as well as parts of the livers were processed for immunofluorescent studies. The chicken sera were also tested for Au(1) by the electro-osmodiffusion technique of Bedariga *et al.*³.

Three hundred wild mosquitoes were collected from the dormitories of a hostel of a secondary school where Au(1) was present in 6.7% of the 423 boarders. Fifty of these were processed for immunofluorescence by dissecting and teasing out the salivary gland and the gut and making a smear, air drying and fixing with acetone at 25° C before doing the test. The remaining 250 were each homogenized in 0.1 ml. of PBS (pH 7.5) and the supernatant after settling under gravity used for the electro-osmodiffusion test.

Only the *Culex pipiens fatigans* mosquito fed on the infected blood. The *Anopheles* and *Aedes* species refused to feed on this blood and died of starvation, although the controls fed on blood without Au(1). Australia antigen was shown to be present in the lumen of the gut of the mosquito immediately after feeding on blood or serum containing Au(1). The antigen persisted in the lumen for 10 days, after which it was no longer detectable in any tissue. After 3 weeks, the antigen reappeared in the salivary glands. Chickens bitten thereafter had Au(1) in the subcutis. Au(1) was never detected in the liver or blood of the chickens.

None of the fifty mosquitoes caught from the school premises showed fluorescent Au(1) either in the lumen or salivary gland. But one of the 250 mosquitoes tested by homogenizing and electro-osmodiffusion had Au(1).

The hypothesis that mosquitoes could serve as biological vectors for Au(1) has been confirmed by this study. It seems that, once infective, culicine mosquitoes remain so for life, for some of the infected mosquitoes that died after 8 weeks still had Au(1) both in the lumen and in the salivary gland. We are unable to explain the *Anopheles* and *Aedes* mosquitoes' consistent failure to feed on blood containing Au(1). Controls of these same species, however, readily fed on blood not containing Au(1). Studies are in progress to see if the Australia (Au(1)) antigen can be detected in the salivary gland by electron microscopy and also if cells cultured from the larvae of *Culex pipiens fatigans* can sustain the Au(1) antigen and lead to its replication.

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Inducible Antibacterial Defence System in *Drosophila*

IN spite of an extensive literature^{1,2} little is known about the way in which insects defend themselves against bacteria. Antibodies have not been found in animals more primitive than certain species of fish^{3,4} and organ transplantation is

possible in larvae of several species of Lepidoptera⁵ unopposed by defensive systems such as found in vertebrates.

We describe here a host-parasite system suitable for quantitative studies of an inducible defence mechanism in an insect. Since a further aim was to develop a double genetic system in both host and parasite, we used as host *Drosophila melanogaster* strain KAS, inbred by pair-matings for more than sixty generations. As parasites we used antibiotic-resistant strains of *Pseudomonas aeruginosa*, *Escherichia coli* K12 and *Aerobacter cloacae*.

We were interested in conditions in which infection is not transferred from one fly to another, and therefore injected 10² to 10⁵ viable bacteria (in about 0.25 μ l.) into the abdomen of *Drosophila* males 1–4 days old, using the standard technique for chemical mutagens. It took about 10 min to inject ten flies. At different intervals the bacterial titre was determined in a homogenate of (usually) ten flies. Serial dilutions were made and aliquots were spread on agar plates with rich medium, containing appropriate antibiotics to suppress contamination from the normal bacterial flora of the flies.

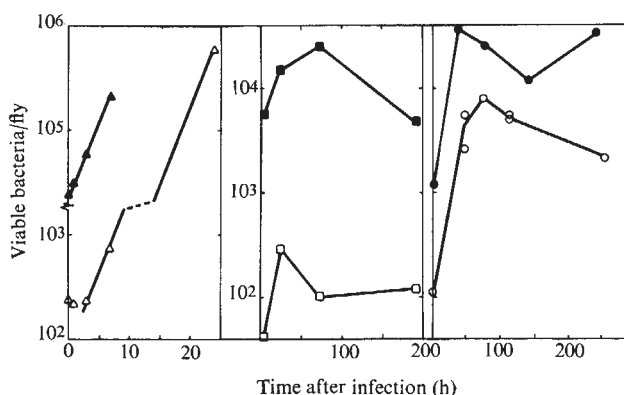


Fig. 1 Primary infection of male *Drosophila melanogaster* in which different doses of bacteria were injected into the abdomen. Flies were maintained at 25° C and at different times aliquots of ten were homogenized with 100 μ l. of 0.9% NaCl and the bacterial titre determined. Flies injected with 240 viable *P. aeruginosa* strain a1 (Δ), flies injected with 2.5×10^4 viable *P. aeruginosa* strain a1 ($\blacktriangle$), in both cases assayed on plates containing ampicillin (10 μ g/ml.). Flies injected with 230 viable *E. coli* D31 ($\square$), flies injected with 2.3×10^4 viable *E. coli* strain D31 ($\blacksquare$), in both cases assayed on plates with streptomycin (100 μ g/ml.). Flies injected with 250 viable *A. cloacae* strain β 1 ($\circ$), flies injected with 10^4 viable *A. cloacae* strain β 1 ($\bullet$), in both cases assayed on plates with penicillin G (7.5 μ g/ml.). With the lower inoculum of strain β 1 double points were taken at 48 and 115 h.

The bacteria used gave different types of dose-response when flies maintained at 25° C were injected with different inocula (Fig. 1). With a very small dose of *P. aeruginosa* strain a1 (about 100 per fly), the bacteria multiplied until they reached 10⁶–10⁷ per fly, when all flies were killed. If the dose was higher the flies died earlier but the growth rate of the bacteria was the same. Several strains of *P. aeruginosa* were tested and all continued to grow even after the death of the flies.

Different doses of *A. cloacae* β 1 gave growth curves with similar or slightly varying plateau levels. With *E. coli* K12 strain D31 varying inocula gave rather little bacterial growth, so that the curves had different plateau levels. The requirement of strain D31 for proline, tryptophan and histidine is irrelevant to the results because similar curves were obtained with strain D31 and with strain D36, a derivative of D31 from which the auxotrophic markers had been removed by transduction. We also investigated two auxotrophic strains of *P. aeruginosa* (OT106 requiring isoleucine and OT97 requiring tryptophan and histidine) and found that they grew to about 10⁵–10⁶ bacteria per fly, at which concentration all infected flies were killed. These control experiments would exclude that growth factor requirements have influenced the results shown in Fig. 1.

Since the flies had been kept together in groups of ten, it was important to rule out that a fatal infection caused by injection of one fly could affect another fly which otherwise would survive. Normal flies were therefore exposed either to food infected with *P. aeruginosa* or to flies that had died from injections with this bacterium. These treatments produce no fatal infection although the flies carried the organism.

According to Steinhaus², some investigators "immunized" caterpillars against *E. coli* while others failed to vaccinate a cockroach against *P. aeruginosa* or *E. coli*. The vaccines used were either heat-killed bacteria or cultures "three months old or older". We have tried to make vaccines from the three bacteria tested in Fig. 1. Heat-inactivated cells of *P. aeruginosa* offered no protection against a secondary infection with the same organism. *E. coli* cells, either viable or partially killed by freezing and thawing, also gave negative results. *A. cloacae*, however, functioned as vaccine. The bacteria were grown exponentially in the rich LB medium⁶, collected by centrifugation, washed and resuspended in 0.9% NaCl at a density of about 10^9 viable bacteria per ml. Such cells could be used as such or after rapid freezing and thawing at -40°C and $+30^\circ\text{C}$, respectively. (Frozen batches of vaccine are often less effective than living cells. A single freezing and storing at -70°C of aliquots of *A. cloacae*, however, provides multiple samples of uniform vaccine from a single bacterial culture.)

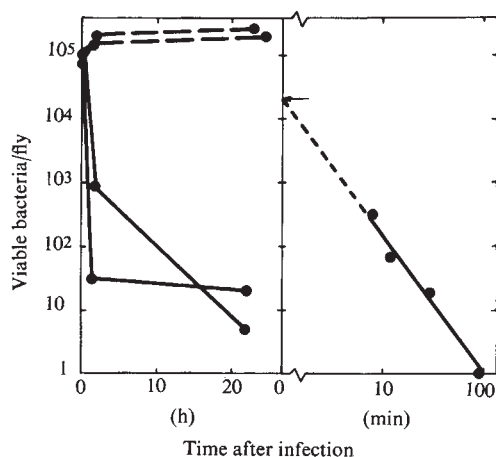


Fig. 2 Three separate experiments with secondary infections of *Drosophila* using *A. cloacae*, strain $\beta 11$. As vaccination the flies were injected with a frozen and thawed sample of strain $\beta 11$. Left part: two experiments with vaccinated flies (solid lines) and two control experiments with normal flies (dotted lines). At different times samples of 10 flies were homogenized, aliquots were diluted and spread on plates with streptomycin (100 $\mu\text{g}/\text{ml}$). Right part: a double logarithmic plot of an experiment with similarly vaccinated flies in which the first sample was taken at the shortest possible time (7 min). The extrapolation to the arrow corresponds to the estimated dose injected.

The left part of Fig. 2 shows the results of two experiments in which vaccinated and non-vaccinated *Drosophila* were challenged by infection with a streptomycin-resistant mutant (strain $\beta 11$) derived from the *A. cloacae* (strain $\beta 1$) used for the vaccination. The zero time concentration was estimated from viable counts on the culture used for the infection. Other points were obtained by homogenization of ten flies for each time point. The results show that within 1–2 h there was a 200 to 5,000-fold reduction in the bacterial titre. After about 24 h the unvaccinated flies contained more than 10^5 bacteria per fly while in the vaccinated ones infection was almost eliminated.

In these experiments vaccinated flies received two injections while controls received one, but in control experiments we injected first 0.9% sodium chloride and then strain $\beta 11$. The result was the same as for primary infections. It can therefore be ruled out that injection as such is responsible for the vaccination effect.

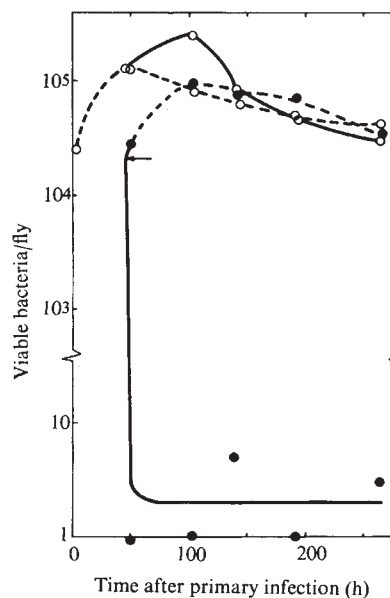


Fig. 3 The amount of *A. cloacae* in normal and vaccinated *Drosophila*. The first infection (the vaccination) at zero time was with *A. cloacae*, strain $\beta 1$ (○---○). Flies receiving a secondary infection with strain $\beta 11$ (solid lines), viable counts for $\beta 11$ on plates with streptomycin (●—●), total viable counts for both $\beta 1$ and $\beta 11$ (○—○). Arrow shows time of infection with $\beta 11$. The corresponding primary infection with $\beta 11$ (●---●).

We have tried to investigate the kinetics of the defence mechanism. The right part of Fig. 2 shows an experiment in which about 10^4 bacteria were eliminated within 100 min. In the double logarithmic plot used the points fall near a straight line which for zero time extrapolates to the estimated dose injected. This result is inconsistent with a first order reaction and implies a complex mechanism.

In other experiments we investigated whether the defence system is specific for the organism used in the vaccination. Table 1 shows that flies vaccinated with *A. cloacae* $\beta 1$ were protected against *A. cloacae* $\beta 11$ as well as against the strains of *P. aeruginosa* and *E. coli* used in Fig. 1. These results could therefore be taken as evidence that the defence mechanism is lacking a high degree of specificity. Other experiments, however, indicate that the defence mechanism can discriminate between two closely related strains of *A. cloacae*. Strain $\beta 1$ was used for the primary infection (the vaccination) while the streptomycin-resistant mutant $\beta 11$ was used 48 h later for the secondary infection (to test the effectiveness of the defence mechanism). Flies were homogenized at different times and

Table 1 Comparison of Normal and Vaccinated *Drosophila* after Infection with Three Different Bacteria

Bacteria used for infection	Normal <i>Drosophila</i> Time of sampling (h)	Viable bacteria/fly	Vaccinated <i>Drosophila</i> Time of sampling (h)	Viable bacteria/fly
<i>A. cloacae</i> , strain $\beta 11$	3	3×10^4	2	<5
	57	1×10^5	57.5	<5
	95.5	8×10^4	93.5	<5
	170.5	7×10^4	168	<5
<i>E. coli</i> K12, strain D31	0.5	4×10^2	0.5	<5
	54	3×10^2	54	<5
	99	7×10^2	99	<5
<i>P. aeruginosa</i> , strain $\alpha 1$	0.5	2×10^2	0.5	30
	23	10^6 – 10^7	23	34
			44.5	<10

As vaccination flies were injected with a frozen and thawed sample of *A. cloacae*, strain $\beta 1$. The time between the vaccination and the secondary infection was two days with *E. coli* and *A. cloacae* but four days with *P. aeruginosa*. With the latter organism all normal flies had died after 23 h, among the 30 vaccinated flies eight died. The dead vaccinated flies were not used since bacteria grow in them.

aliquots were diluted and spread on plates which would allow both $\beta 1$ and $\beta 11$ to grow, as well as on selective plates which would allow only strain $\beta 11$ to grow. Fig. 3 shows that the defence mechanism eliminated the streptomycin-resistant mutant $\beta 11$ used as challenger while it allowed the parental strain used as vaccine to persist at a steady state level close to 10^5 bacteria per fly. We also used strain $\beta 11$ as vaccine and found that a nalidixic acid-resistant mutant (strain $\beta 12$) used for the second infection was eliminated. Thus, the specificity of the vaccination effect is not due to a special relationship between $\beta 1$ and $\beta 11$.

To estimate quantitatively the capacity of the induced defence system, we followed the life span of flies given varying doses of *P. aeruginosa*. Fig. 4 shows that vaccinated flies were killed by 2×10^5 bacteria per fly, but their life span was prolonged by about 20 h. The lowest dose tried, 2×10^3 bacteria per fly, allowed about 50% of the flies to survive for more than 70 h. The result was confirmed in another experiment using lower doses of *P. aeruginosa*. These conditions therefore reflect the maximum capacity of the defence system induced by the actual vaccine. It should be stressed that the capacity of the defence system to kill a given number of bacteria depends on the virulence of the organism used.

Another approach to assay for the defence mechanism was to use an *in vitro* reaction. We compared homogenates of vaccinated and normal *Drosophila* for their effect on living cells of *A. cloacae* strain $\beta 11$. Twenty flies were homogenized with 50 μ l. of 0.9% NaCl containing 100 μ g/ml. streptomycin. Each sample received 4×10^4 viable bacteria in 5 μ l. The mixture was incubated at 30° C and at different times samples of 5 μ l. were withdrawn, diluted and plated. Fig. 5 shows that in the homogenate from the vaccinated flies there was at least a twenty-fold reduction in the number of viable bacteria while in the control there was a moderate increase. The results show that an *in vitro* killing reaction can be demonstrated but in the conditions used it was far less effective than the defence system *in vivo*.

It is difficult to compare our findings with those described using other insects and only semi-quantitative biological techniques^{1,2}. Our results show, however, that the use of antibiotic-resistant bacteria together with selective media is a convenient procedure and that the organisms selected form suitable combinations for further studies of host-parasite relations in insects. Vaccinated flies should offer good opportunities to select bacterial mutants resistant to the defence system.

Although few investigators have made use of *Drosophila*^{1,2}, infections with *P. aeruginosa* and *E. coli* have been studied with several other insects. The general observation has been that *P. aeruginosa* is highly virulent for insects while *E. coli* is tolerated in large doses^{1,2}. Our results show that in these respects *Drosophila* resembles other insects.

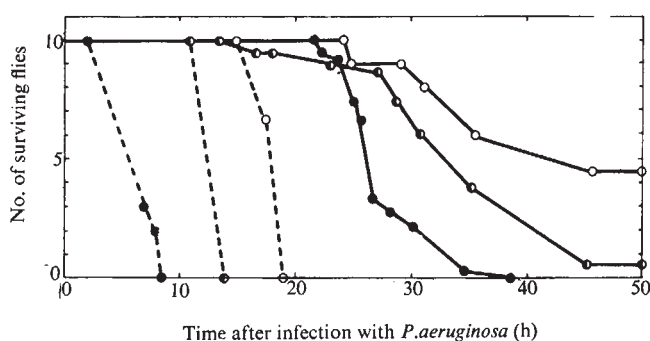


Fig. 4 Prolongation of the life span of *Drosophila* after vaccination with *A. cloacae*, strain $\beta 1$. Three different doses of *P. aeruginosa*, strain $\alpha 1$, were injected, namely 2×10^3 viable bacteria/fly ($\circ$); 2×10^4 viable bacteria/fly ($\bullet$); 2×10^5 viable bacteria/fly ($\bullet$). Controls with untreated flies (---); vaccinated flies (—).

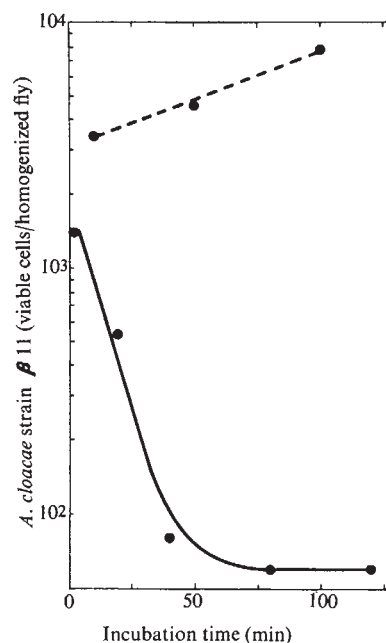


Fig. 5 Assay for bactericidal effect in homogenate from normal *Drosophila* (---) and from flies vaccinated with *A. cloacae*, strain $\beta 11$ (—). Incubation was performed at 30° C and aliquots of 5 μ l. were withdrawn at the times indicated.

Although the nature of the inducible defence system is not known, a few comments can be made. First, the inducer can only be a vaccine that contains a relatively high number of non-virulent, living bacteria. The phenomenon is therefore most likely different from both Stephens's vaccination of wax moth larvae by heat-killed *P. aeruginosa*⁷ and Bakula's antibacterial factor in *Drosophila* larvae⁸. It is relevant that none of these systems showed any antibacterial activity against *E. coli*.

Second, the fact that *A. cloacae* is suitable for induction of the defence system could be related to the finding that this bacterium persisted in the fly at a steady state level for as long as 10 days. Preliminary experiments indicate that the induction mechanism is inhibited by cycloheximide but not colchicine.

Third, the defence system induced in *Drosophila* can discriminate between two mutants of *A. cloacae* and at the same time act against *P. aeruginosa* and *E. coli*. This means that the fly together with the first infecting strain of *A. cloacae* (the vaccine) form a "self" which reacts against any second infection by "not-self". This cell symbiosis may resemble the specificity which allows only the first homologous spermatozoa to penetrate an unfertilized egg while all subsequent ones are rejected. It could also be related to the special ability some insects may have to react against the second of two consecutive parasite eggs⁹.

Finally, the specificity of the *Drosophila* defence system seems to fit into the pattern for the evolution of "self-recognition" as discussed by Burnet¹⁰.

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Treatment of Fruit with Propylene gives Information about the Biogenesis of Ethylene

FLESHY fruits have been divided into two classes on the basis of their respiratory behaviour during ripening: climacteric fruit, such as bananas, which undergo a large increase in respiration (climacteric rise) accompanied by marked changes in composition and texture, and non-climacteric fruit such as citrus, which show no changes in respiration that can be associated with distinct changes in the composition of the fruit¹. An increase in the level of endogenous ethylene is considered to be the immediate trigger of ripening in climacteric fruits². Fruits of this class usually produce large amounts of ethylene once ripening is under way. They may also be induced to ripen by treatment with ethylene at concentrations above about 0.1 p.p.m. for a suitable period³. The ripening induced by exogenous ethylene has been considered to be qualitatively identical with that which occurs naturally³. In both cases, once ripening is induced it has been considered that endogenous ethylene production rises autocatalytically⁴. Uninjured citrus fruit have been shown to produce low amounts of ethylene⁵. Their respiration may be increased by treatment with ethylene⁶ and disappearance of chlorophyll (colouring) and ageing may be more rapid¹⁸.

There seemed to have been no previous attempts to examine the influence of exogenous ethylene on concomitant endogenous production of the gas either in climacteric or in non-climacteric fruits. Vendrell and McGlasson⁷, however, have shown that endogenous production of ethylene by banana fruit may be

suppressed following treatment with 5–10 p.p.m. ethylene. In this report we show that treatment of fruit with propylene, the most active analogue of ethylene⁸, enables the measurement of endogenous ethylene production during the ensuing increase in respiration.

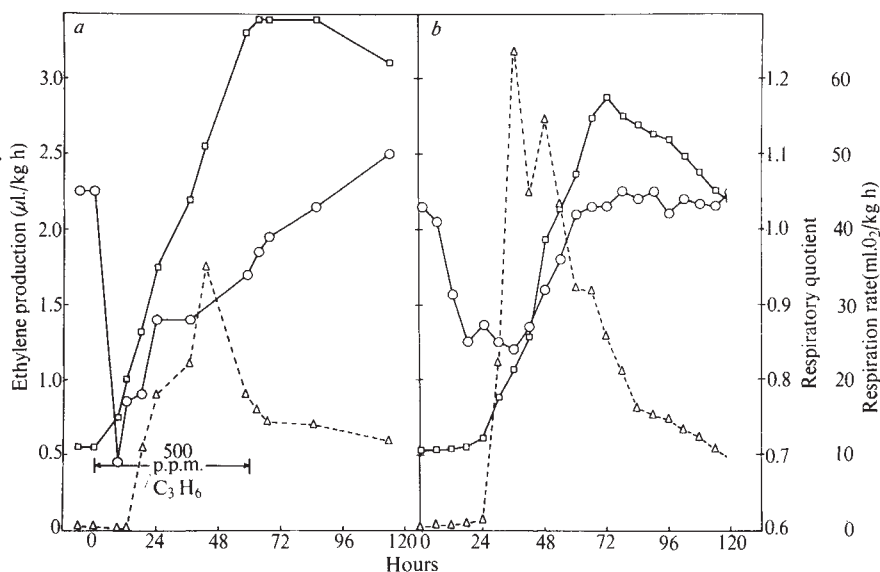
Green bananas (Williams cultivar of the Intermediate Cavendish group), light green lemons (cultivar Lisbon) and mature oranges (cultivar Valencia) were obtained from commercial plantings and ventilated with streams of ethylene-free humidified air at known flow rates. Propylene was added to the air streams at 500 p.p.m. and the duration of treatment was 2.5 days. According to Burg and Burg⁸, this concentration is equivalent to about 5 p.p.m. ethylene, an amount previously shown to give about optimum suppression of endogenous ethylene production in banana fruit⁷.

The concentrations of ethylene were measured with a flame ionization gas chromatograph fitted with a fractionating column packed with alumina⁹. The retention times were 1 and 4 min for ethylene and propylene respectively. The undiluted commercial propylene used contained only a trace of ethylene. Oxygen uptake was measured with a paramagnetic oxygen analyser and CO₂ production was measured with an infrared gas analyser¹⁰.

Treatment of green bananas with propylene resulted in a typical climacteric pattern of respiration (Fig. 1a and b). The rise in respiration was detected about 10 h after the beginning of treatment. This rise was accompanied by a decrease in the respiratory quotient (RQ) from the preclimacteric value of 1.05 to about 0.70. The RQ remained less than 1 until near the climacteric maximum as reported by Brady *et al.*¹¹. Treated fruit ripened normally. An increase in endogenous ethylene production was not detected until more than 3 h after the beginning of the respiratory rise (Fig. 1a). In contrast both respiration and endogenous production in naturally ripened fruit began to increase slowly at least 18 h before the main burst in respiration signified the onset of the climacteric rise (Fig. 1b). This period of slowly increasing respiration was marked by a decrease in the RQ. The main burst in respiration was accompanied by a rapid increase in ethylene production. In relation to the climacteric peak maximum production of ethylene in propylene treated bananas occurred about 12 h later than in naturally ripened fruit. Post-climacteric ethylene production by treated and untreated fruit was similar.

An increase in oxygen uptake and a drop in RQ were detected in both lemons and oranges about 10 h after the commencement of treatment with propylene (Fig. 2a and b). Respiration rates reached a broad peak after about 24 h and then remained steady until about 60 h. The rates subsequently

Fig. 1 Rates of respiration and ethylene production, and respiratory quotients (RQ) of bananas. *a*, Treated with 500 p.p.m. propylene, s.e. of means ($n=3$): respiration 58–114 h ± 0.92 (12 d.f.), other times ± 1.2 (24 d.f.); ethylene production ≤ 1 $\mu\text{l./kg/h} \pm 0.076$ (6 d.f.), < 1 $\mu\text{l./kg/h} \pm 0.056$ (33 d.f.); RQ < 0.7 and $> 1.0 \pm 0.038$ (12 d.f.), ≥ 0.7 and $\leq 1.0 \pm 0.011$ (24 d.f.). *b*, Control, s.e. of mean ($n=3$): respiration 66–84 h ± 8.9 (6 d.f.), other times ± 1.9 (36 d.f.); ethylene production < 1 $\mu\text{l./kg/h} \pm 0.031$ (24 d.f.), > 1 $\mu\text{l./kg/h} \pm 0.47$ (18 d.f.); RQ ± 0.015 (42 d.f.). Control fruit began to ripen about 26 days after harvest (zero hours in part *b*). Δ , C₂H₄; $\square$, O₂; $\circ$, RQ.



declined and the RQ gradually returned to the pretreatment levels. In contrast to bananas, endogenous ethylene production in propylene-treated lemons and oranges remained at low levels in the presence of propylene. Even when held subsequently for several days in the absence of added propylene endogenous ethylene production failed to rise in either lemons or oranges, although the former became fully yellow.

We have not attached any metabolic significance to the rapid decrease in RQ after the application of propylene to the fruit. A similar drop in RQ in ethylene-treated banana fruit was reported by Brady *et al.*¹¹, and has also been observed by us in other fruits undergoing a fairly rapid increase in respiration (unpublished work). The drop in RQ is thought to be due to differences in the rates of diffusion of CO₂ and O₂ through fruit tissue.

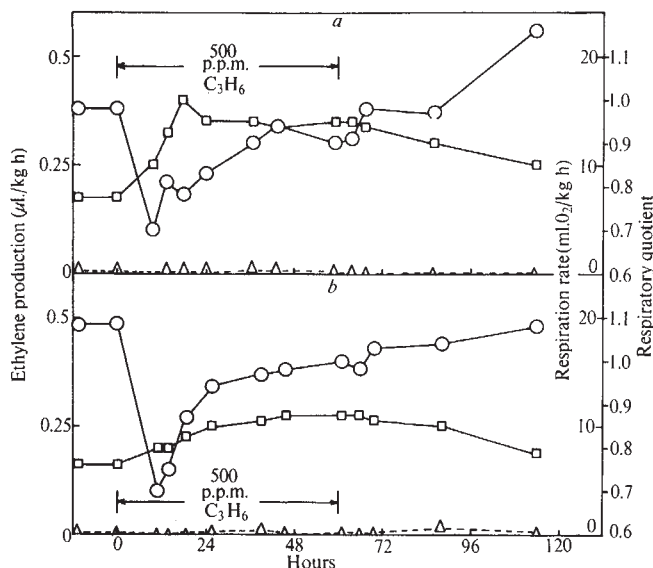


Fig. 2 Rates of respiration and ethylene production, and respiratory quotients (RQ) of lemons and oranges treated with 500 p.p.m. propylene. *a*, Lisbon lemons, s.e. of means ($n=4$): respiration ± 0.68 (36 d.f.), ethylene production ± 0.0026 (39 d.f.), RQ ± 0.040 (33 d.f.). *b*, Valencia oranges, s.e. of mean ($n=4$): respiration ± 0.77 (36 d.f.), ethylene production ± 0.0031 (39 d.f.), RQ ± 0.041 (36 d.f.). Δ , C₂H₄; $\square$, O₂; $\circ$, RQ.

Our results provide additional insights into the factors which regulate ethylene biogenesis in fruits and the differences which distinguish climacteric from non-climacteric fruits. We suggest that in the banana fruit two systems regulate ethylene biogenesis and that the sequence of events is as follows. In the naturally ripening fruit ethylene biogenesis is initiated by some factor involved in the regulation of ageing processes (system 1). The ethylene so produced reaches a level in the fruit sufficient to initiate the respiratory climacteric and the associated ripening changes. The rise in respiration is accompanied by a further large increase in ethylene production (system 2). Quazi and Freebairn¹² have provided evidence that this apparent excess of ethylene is required for the full integration of the various changes associated with natural ripening.

When propylene or ethylene is added to bananas, ethylene production by system 1 is circumvented but production by system 2 is initiated, hence the delay in the rise and the peak of endogenous ethylene production relative to respiration (Fig. 1). System 2 is, however, subject to some suppression by the continued presence of a high concentration of propylene or ethylene. This suppression apparently involves mechanisms other than a simple end-product inhibition because suppression has been shown to continue even after the end of treatment with ethylene⁷.

An increase in cell permeability has frequently been invoked

to explain the initiation of the respiratory climacteric and ripening, and presumably also system 1 ethylene production¹³. Brady *et al.*¹¹, however, have clearly shown that these processes are not necessarily associated with a change in cell permeability. When banana slices were treated with ethylene an increase in cell permeability was not detected until about 28 h after the beginning of the respiratory climacteric although in naturally ripened tissue an increase in permeability was detected 3–4 days before the onset of the respiratory climacteric.

Although it seems unlikely that ethylene acts directly by increasing cell permeability it is still possible that ethylene exerts a more subtle effect on an enzyme system bound or associated with a membrane³. This interpretation is supported by the well known observations that it is necessary to apply ethylene at about 10 p.p.m. for at least 24 h to stimulate a normal climacteric pattern of respiration and ripening in immature fruit of the climacteric type³. If ethylene is applied for shorter periods respiration returns almost to pre-treatment levels and there is little stimulation of ethylene biogenesis⁷. The lag in the onset of ethylene biogenesis relative to the rise in respiration in propylene-treated bananas could also indicate the induction of a specific enzyme required for ethylene biogenesis.

Lemons and oranges apparently lack system 2 since propylene failed to stimulate endogenous ethylene production. The absence of an increase in ethylene production is also an indication that the respiratory rise induced by propylene was not an injury response. Citrus fruit are known to evolve ethylene at high rates after various treatments, including application of various salts¹⁴, and the inhibitor of protein synthesis, cycloheximide¹⁵. The failure of propylene to stimulate endogenous ethylene production provides an explanation for the observations that respiration in citrus fruit may be repeatedly stimulated by intermittent exposures to ethylene¹⁶. Thus the changes induced by ethylene in citrus are reversible but they are not reversible in banana fruit. Burg and Burg⁵ and Reid and Pratt¹⁷ have suggested that the critical difference between climacteric and non-climacteric fruits rests in their relative abilities to produce ethylene in response to low concentrations of ethylene. Our results support this suggestion.

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BOOK REVIEWS

Social Sickness

Society, Stress and Disease. Vol. 1. (Proceedings of an International Interdisciplinary Symposium held in Stockholm, 1970.) Edited by Lennart Levi. Pp. xvi+485. (Oxford University Press: London, New York and Toronto, September 1971.) £8.

THE purpose of the series of symposia of which the first is reported in this volume, was to define those features of the psychosocial environment likely to be related to the "psychosomatic" diseases or, in Kagan's words, to establish "whether psychosocial stimuli cause disease through the mediation of stress, and, if so, under what circumstances and in what diseases and how this may be prevented without losing the benefits of such stimuli".

In territory so ill-charted and largely unconquered, it might have been predicted that the discussions would range far and wide. Not only disease but delinquency, crime, violence, pollution and the population explosion come under scrutiny. Symposiasts also take in their stride the communication feedbacks that help to maintain social homeostasis in democratic societies, and the disturbing isolation and aura of sanctity that divorce Presidents of the United States from political realities. Erich Fromm describes the paranoid and fanatical thinking of governments and military bureaucracies which foments international strife and, in the USA, according to Corson, helps to perpetuate racial conflicts and the Vietnam war. Among environmental factors brought into view are the rapidity of social change and the alienation and dwarfing of individuality engendered by large-scale industrial production and technological change. Also encompassed are overcrowding, the lack of social cohesion of modern, urban communities, the repetitiveness and forced pace of work, noise and air pollution which make necessary planning of the physical environment and social organization better suited to human needs. Weighed down by heavy diet, one is ill-prepared for the trapeze acts often needed to proceed from one

chapter to the next. But the fare is nutritious. A large body of up-to-date information is assembled between the covers; a number of the contributions and exchanges are fresh, lively and stimulating. The volume makes a valuable source book.

The big city, where crime, violence, drug dependence and alcoholism seem, with other ills, to proliferate, receives a lot of attention. Greater Stockholm with 16% of Sweden's population is responsible for 39% of all armed robberies committed. In such overcrowded conditions, man behaves like his primate ancestors, a point often made in recent years by ethologists and here restated by Hamburg. The built-in mechanisms for curbing violence are deranged and destructiveness is unleashed. An experiment by Zimbada is cited to illustrate the weakening of the sense of mutual obligation and involvement engendered. A car was abandoned in a middle-class neighbourhood near a large university campus in New York and an identical exercise was conducted in Stanford which, being smaller, possibly fosters concern and cooperation. In New York the dismantling, carried out mostly in daylight by white middle-class people, began within 7 minutes and after 64 hours the car was a "battered useless hulk of metal the result of twenty-three incidents of destructive contact". The Palo Alto car was not only unharmed but a passer-by obligingly lowered the hood when it began to rain.

Little progress is made in disentangling the multitude of factors involved or in separating causes from effects. Comparable segments of New York and Stanford would, for example, probably differ not only in population density but also in respect of measures of "social disorganization" which emerges from Leighton's work as associated with psychiatric disorder. Nor would their citizens be likely to prove identical in respect of indices of "social incongruity" which "arises when changes demand adaptation that is not made or is inadequate", and which Kagan hypothetically links with psychosomatic disease; like other possibly

relevant factors this is embedded in a host of others. As it is the second generation of immigrants that becomes delinquent, Carlestam rejects the possibility that inherently aggressive and predatory persons are more likely than others to be drawn to the city. But this ignores the well-established deviation from the norm both of immigrants and the parents and even grandparents of delinquents.

The questions of greatest clarity and promise for scientific advance arise in relation to specific disorders in specific settings — the conglomeration of psychiatric disorders and defects associated with economic underprivilege and social disorganization, the differences in prevalence of cardiovascular disease observed in Americans, Japanese, Zulus and Yemenite Jews in different socio-cultural settings, the excess in mortality from suicide, road accidents, cirrhosis of the liver and pulmonary tuberculosis among survivors of concentration and prisoner-of-war camps as compared with controls. Knowledge will probably advance as before by extension from understanding of simpler to more complex systems.

Public health and planning authorities and governments would do well to take note of Kagan and Levi's thoughtful, lucid paper on practical measures which lays no claim to scientific authority. But when it comes to action on a world scale to avert catastrophe, irreconcilable conflicts appear to present at every level. One example must suffice. The thunder of the sages, who attacked the industrial revolution in its earliest years for its dehumanization and destruction of freedom and individuality, re-echoes in these pages. But underdeveloped countries will view with justifiable suspicion the view that Indian peasants and Egyptian fellaheen are more free than the citizens of London and Tokyo. They may also be inclined to regard exhortations to renounce the technological developments of affluent societies lest life on Earth should become insupportable as "pull up the ladder, Jack" arguments. Moreover, improved living standards have regu-

larly been associated with declining birth rates and unless the population explosion can be contained we shall all be undone.

One can perhaps hope that those who have power to plan and decide will, at the least, take into account such facts, predictions and informed guesses as scientists are able to offer. Among other things, they may note the instruction that Deasy puts into these words, "Since the quality of the social and psychological milieu is far more crucial to human performance than the quality of the plumbing—it is clear that the people who know about the planning and the people who know about people should get together."

MARTIN ROTH

Partial Belief

The Matter of Chance. By H. H. Mellor. Pp. xiii+190. (Cambridge University: London, November 1971.) £4; \$13.

ACCORDING to Kipling there are nine and sixty ways of constructing tribal lays, and every single one of them is right. He might have said the same thing about theories of probability. Mr Mellor thinks that his own view of the subject "makes more sense than its philosophical competitors do of what professional and everyday usage assume to be true of chance". It remains to be seen whether philosophers and statisticians will share his opinion.

A long line of theologians and scientists of a deterministic persuasion have regarded chance as merely a convenient word to describe human ignorance about the causality of situations, something which is unknown to Nature herself. Mr Mellor does not commit himself very deeply on this issue but he appears to consider chance, if not as a thing in itself, at least as something with which objective reality can be associated. And therein, of course, lies the problem.

At the extreme ends of an almost continuous spectrum of possible views about the nature of probability lie the frequentist, for whom it is a measure of relative frequency of occurrence, and the subjectivist, for whom it is a measure of belief (or some attitude of mind). Mr Mellor rests on what he calls "partial belief" in propositions. This is "a disposition to indulge in publicly describable behaviour, for example, specifically in betting situations". One may leave it to the psychologist or the philosopher to argue whether there is any profound semantic content in such a definition. One may concede that some link between partial

belief and behaviour is necessary on the way to a theory in which chance has any kind of objectivity.

The advantage of a subjective view about chance is that it enables us to speak of the probability of a single non-repeatable event, a point on which the frequentist is in trouble. The disadvantages are in setting up a measure of such probability and in justifying any claims that such a measure has anything to do with the way things happen. Jeffreys took the bull by the horns by postulating that probabilities—or at any rate the kind of probabilities he wanted to talk about—are orderable. More recent attempts to calibrate the probability scale, dating back to Frank Ramsey (and possibly earlier), rely on considering what sort of odds one would accept in a bet on the proposition in question. This procedure is adopted by Mr Mellor.

The idea is attractive but, in my opinion, unsatisfactory. If I am asked to bet on the proposition that there will be a colony on Mars in a hundred years' time I refuse, simply because I shall not be here to collect or to pay up. If I lower my sights and am asked to bet whether there will be a manned station on the Moon in five years' time, I might venture a modest pound, but I certainly would not bet £10,000, whatever the odds. In fact, if we are to relate probability to the willingness of an individual to accept given odds, we must take account of the size of the stake and his age, and possibly other qualities as well, such as his luck on previous occasions. And in such a case the judgment in probability becomes multidimensional and a single numerical measure is impossible.

Mr Mellor also has a more favourable view than I about the extent of the agreement which is found among different individuals about the intensity of partial belief in any given case. Anyone who has taken part in an exercise of the Delphi type (in which a group selects a set of contingencies and tries to put probabilities on them) knows what wide divergences of opinion emerge. It is true that such exercises are usually concerned with macro-events such as the probability of an atomic war or the possibility of farming the sea commercially. There would undoubtedly be closer agreement about propositions concerning the fall of dice, the risk of death or indeed any member of a collective of events. But here, I think, the frequentist would rear his head again.

Popper tried to meet one of the difficulties in setting up a probability theory by elevating "propensity" into a basic concept. "Propensity" for Mr Mellor is a dispositional property. To a simple-minded statistician to say that the chance of a coin coming down

heads is one half sounds very like saying that it has a propensity to do so measured by a chance of one half. Mr Mellor distinguishes between the two. Propensity is a property of the coin. Chance depends both on the coin and the mechanism used for tossing it. This may make for clearer thinking. It is not evident to me that it contributes much to the professional use of statistical probability.

I have a dispositional propensity to a partial belief (quantifiable at about 0.98) that we shall not plumb the depths of the foundations of probability until we know a great deal more about how the mind works. In the meantime Mr Mellor's views are entitled to attention as a serious assault on a very intractable problem. M. G. KENDALL

Public Enterprise

Public Enterprise in Practice. By Richard Pryke. Pp. xiii+530. (McGibbon and Kee: London, 1971.) £6.95.

THE length and price of this book are likely to deter all but the most ardent students of the nationalized industries. This would be a pity because Mr Pryke has written a very readable, if somewhat controversial, account of the progress of Britain's state industries since their establishment. The study provides a mine of factual and statistical information together with comment and appraisal of the performance of the state sector. It concentrates in particular on productivity and investment of the public sector though there are substantial sections relating to the more important problems faced by particular nationalized industries. It is in no sense an introductory text, however, and many readers may find the course hard going if they have no background knowledge of the subject under discussion.

The study does, however, suffer from some shortcomings. The book is over-lengthy given the single objective at hand and that is to prove that nationalized industries are best. Mr Pryke makes every effort to put the best case forward for the public sector industries. His two main conclusions are that they have been technically more efficient than private enterprise and that their operation has not led to any serious misallocation of resources. He then reasons that "the relationship between good behaviour and public ownership must henceforth be regarded as one of cause and effect". The latter conclusion is certainly debatable and does not follow automatically, but the first two conclusions are not fully substantiated by the analysis. The productivity record of the nationalized industries has been

patchy, especially in the first decade of public ownership, and the subsequent improvement was partly to be expected when the nationalized concerns had sorted out their organizational problems. Moreover, the way in which the productivity estimates are calculated is not stated very explicitly in the text so that it is difficult to judge how reliable they are. When Mr Pryke ventures into the realms of economic analysis, especially in relation to investment allocation, one begins to have further doubts. The level of analysis is scarcely very profound, in fact at times it barely rises above the journalistic type, and the implied assumption that everything was all right once the nationalized industries had forged ahead with discounted cash flow investment appraisal techniques reflects little recognition that these methods are open to considerable criticism.

Clearly in a volume of this size there are bound to be many particular points which give rise to debate. Mr Pryke has certainly read widely and assembled an enormous amount of information in a very able manner. The style of writing is refreshing and controversial but this adds interest to the subject. Thus despite some defects this is undoubtedly one of the more interesting and informative volumes written on Britain's nationalized industries.

DEREK H. ALDCROFT

Natural Causes of Life

The Origin of Life by Natural Causes. By M. G. Rutten. Pp. xx+420. (Elsevier: Amsterdam, London and New York, 1971.) Dfl. 100; \$29.50.

THIS is not quite a new book. It is an expanded and re-written version of the late Professor Rutten's earlier volume *The Geological Aspects of the Origin of Life on Earth* (1962). It is very considerably expanded; in place of 146 small pages, there are 420 large ones; in place of eleven chapters, there are eighteen. Ten of the eleven original chapters retain their original titles, though updated, with much new information and a more discursive treatment. These ten chapters still deal with essentially geological aspects. Eight chapters are quite new, and bring in chemical, astronomical and biological considerations. Unfortunately, the updating is most uneven. No one who knew Professor Rutten as a geologist would have accused him of being in any way uncritical. Yet that, alas, must be the verdict on this, his last work. And it is a very serious accusation, for the book is a positive danger. Our knowledge of the origin of life is still in a highly speculative state. Neverthe-

less, the student, provided he knows where the areas of controversy lie, can gain much from weighing up the pros and cons. A search for limiting constraints is an important part of the scientific method. But Rutten never attempts to highlight controversial aspects, even when they are still very much to the forefront of present-day research. Quite the contrary. If he mentions them at all, he mentions them as a politician might do, rather than as a scientist. The evidence he quotes against his own point of view is inaccurate and incomplete. Thus, he comes to the astonishing conclusion that the certain identification of contaminants among the "organized elements" in the Orgueil meteorite by Fitch and Anders "has been refuted by now". "Anyone who will take the trouble of comparing the actual preparations under the microscope," he says, "will agree that this resemblance is not more than an apparent one, and that the 'organized elements' have nothing to do with ragweed pollen." He ignores the paper, published in 1963, which gave certain identification of the "Type 5" organized element with ragweed pollen and quotes instead an earlier—and less convincing—paper of the same year. By taking this attitude, he has missed one of the most fascinating lessons that these studies have to teach us. For, with the publication of the evidence of Fitch and Anders, most scientists came to the conclusion that, as some of the organized elements were certainly terrestrial contaminants, all of them could be dismissed as requiring no further explanation. It has only quite recently become clear that they were wrong; some of the organized elements (the very simple and by far the most abundant ones) have proved to be both indigenous and extra-terrestrial. The full-blooded sceptics had been a little too sceptical, and thrown out a whole lot of babies with the bath-water. But Rutten seems utterly naïve, and has retained in his bath-tub all the spurious babies, and even quotes the names they were given in 1962, though these have been abandoned long since.

Yet it would be a mistake to reject this book just because it is dangerously misleading. Though Rutten has repeatedly ignored controversy, he has brought together a vast amount of information in as readable a form as is available anywhere. He has ideas of his own, and he develops them with skill and enthusiasm. He has provided a well-illustrated account of early Precambrian fossils (though not so complete or authoritative as Schopf's own essay, published in *Biological Reviews* for 1970). He has retained from his own former version the logical reasoning that so illumined our view of the early Earth's environment. There have been

many other rival texts on the origin of life published during the past few years. This volume differs from them all. Rutten's strong points are their weak points; Rutten's weak points are strong in them. For example, *Biochemical Predestination* by D. H. Kenyon and G. Steinmann (McGraw-Hill, 1969) makes (except in the matter of carbonaceous meteorites, which they omit altogether) an excellent complement to Rutten. The two books read together give a much sounder picture than either taken by itself.

P. C. SYLVESTER-BRADLEY

Factors in Development

Schering Symposium on Intrinsic and Extrinsic Factors in Early Mammalian Development. Edited by Gerhard Raspe. Pp. 653. (Pergamon: Oxford and New York; Vieweg and Sohn, GmbH: Braunschweig, September 1971.) £12.50.

THIS volume contains the proceedings of the fourth Schering Symposium, held in Venice in April 1970. Although more than 230 participants are listed, the meeting seems to have had something of the character of a workshop, since most of the papers combine an informal account of unpublished work with a review of the field. The discussions at the end of each paper are printed *in extenso*.

The first two sessions, on the oocyte and the egg, and the metabolism of preimplantation embryos, account for fourteen of the twenty-nine papers, and 300 of the 600 pages. They succeed in covering their respective fields in a fairly comprehensive manner, and since both fields are rapidly advancing but still reasonably delimited, this section of the book makes interesting reading.

It is invidious to single out individual authors from such a wealth of talent, but I personally enjoyed very much the papers by T. G. Baker on the electron microscopy of primary and secondary oocytes, and by W. K. Whitten on the nutrient requirements of preimplantation embryos *in vitro*. The topic of abnormal fertilization, well reviewed by C. Thibault in relation particularly to ageing of the egg and sperm, may prove to be of crucial importance in the understanding and ultimately in the prevention of chromosome abnormalities in man.

The remaining four sessions are inevitably more diffuse, since they attempt to deal in a small compass with large subjects: placental and foetal physiology, immunology, differentiation of tissues, and teratology. The scope is no longer confined to mammalian development, since at least two of the papers

deal primarily with Amphibia (H. Tiedemann on induction and cell interaction in embryonic development, and G. M. Maniatis and V. M. Ingram on "foetal" haemoglobins).

Photographs and curricula vitae of the speakers are included, but no subject index. Misprints make some of the tables and figures hard to understand for anyone not familiar with the work. On the other hand, the editing of the discussions has been well done, with only the occasional "Well . . ." or reply to a non-existent comment.

ANNE McLAREN

Geology on the Move

The Great Ocean Business. By Brenda Horsfield and Peter Bennet Stone. Pp. 360+27 photographs. (Hodder and Stoughton: London, January 1972.) £3.50.

THE only really unsatisfactory feature of this book is the rather uninformative and jingoistic title, although it is exceeded in this respect by some of the chapter headings which include "All Change!", "The US Conjuring Trick" and "Where Is Everybody?". These contrast with the book itself which in general avoids journalese and is well written and painstakingly researched. Its fundamental thesis is that oceanography is the new "Big Science", following in the footsteps of atomic physics and space research; or rather that it would be were it not for the general economic recession in the Western world which is affecting not only the amount of money that governments are prepared to spend on scientific research but also the extent to which commerce is prepared to gamble capital investment on new and untried technologies. It makes the case therefore that oceanography as a science has blossomed within the past decade, with spectacular successes in certain areas, and is on the threshold of large-scale expansion, despite financial stringencies, because of its relevance to environmental problems, resources and defence.

A word of warning, however: three-quarters of the book concerns marine geology and marine geophysics and two-thirds of this is devoted to the evolution of the ocean basins in terms of seafloor spreading and continental drift. The justification for this is that it is in this area that oceanography has made greatest strides within the past ten years. Thus physical and chemical oceanography and marine biology get rather lean treatment although the last three chapters, which deal with these subjects, make fascinating reading, concentrating as they do on applied aspects such as fisheries

research, tidal and wave prediction, and pollution in relation to the general circulation and to marine life.

The first four chapters contain an excellent summary of geological thinking and oceanographic exploration before 1960, primarily in relation to the large-scale structural features of the Earth's surface and the ocean floor. The next four chapters relate the development and confirmation of the geological concepts of seafloor spreading and plate tectonics during the 1960s. The treatment is therefore historical and in terms of the individual scientists who had the ideas and made the crucial observations. This story, of the so-called "revolution in the earth sciences", has recently captured the imagination of other popularizers of science and several accounts are imminent; one can only hope that they emulate the accuracy and attention to detail of this one. The geological profession is indeed fortunate and has been well served by the authors of this particularly well-informed account. The only obvious lapse from objectivity is, perhaps inevitably, in chapter 7, where the senior author describes her own contribution to the subject.

The remaining chapters of the book concern more mundane and applied aspects of oceanography—defence related research, the development of manned submersibles as a new and expedient outlet for the US aerospace industry, the various national efforts, and the rapidly expanding field of undersea technology related at present to drilling, primarily for oil and natural gas, and in the future to the mining of manganese nodules and the deep-sea brine sinks.

In some ways the book is two books in one. The first half, describing the revolution in geology within the past ten years, is in many respects an entity in itself; the second half makes the case for oceanography as the new Big Science and the new Big Business: the last frontier on this planet to be explored and exploited. I can heartily recommend it to laymen, students and professionals alike for its readability, veracity and topicality.

F. J. VINE

Invertebrate Compendium

The Invertebrate Panorama. By J. E. Smith, J. D. Carthy, Garth Chapman, R. B. Clark and David Nichols. Pp. x+406+28 plates. (Weidenfeld and Nicolson, November 1971.) £7.25.

THIS is a book ideal to read in the summer between taking A levels and going to a university to study for a degree in zoology. Five well known zoologists have collaborated closely in

its construction. Considering the difficulties of putting such a book together it is remarkably successful. I suppose the decision to make a compendium dictated the pattern of the contents, for I imagine that no single author would have done it quite like this. It is easy to criticize the book as attempting too wide a field. The wide view should clearly be taken in such a book, yet many hands are required to take it because of the very diversity of the subject, and herein lies the dilemma. Taken as initial reading for university students in zoology this is a stimulating book which will lead naturally to those by Barrington and Barnes, and it is to be recommended to libraries of colleges and schools. It is stimulating in giving glimpses of what is known about invertebrates, but being a compendium it is necessarily selective and some other aspects of invertebrate biology which are equally interesting are only touched on or are left out completely. The chapters on the past history and evolution and on the phylogeny of invertebrates come at the end. In a way it might have been more stimulating to have combined these with the initial survey of the "major" and "minor" phyla. Leaving aside the wilfully trendy titles, the division of the phyla into "major" and "minor" as defined here has meaning only with regard to past events and present conditions. Some of the "minor" phyla may have been major in the past, as indeed some were. But if the view of present size is taken as a criterion then I feel a discussion of why their structure or biology allowed them to proliferate into many, or restricted them to few species, should have been discussed in that context. The definition of "major" on the basis of 10^3 – 10^5 living species, and "minor" as those with only 10 – 10^2 species, draws attention to the discrepancy in size between the different groups but the division is uneasy and does not reflect "the major styles or architecture", a description of which forms another chapter. As Dr Smith himself says, "the size of a phylum is of course in no way a measure of its zoological interest". One can only hope that this approach will stimulate rather than confuse. The protists are omitted; and presumably they will form a further complete volume in this series.

The Weidenfeld and Nicolson series is in many ways the successor to the "New Naturalist"—a most successful series published by Collins over more than twenty years—and one must question for whom this present series is intended. Collins's series was clearly intended for the educated layman: all could be read with profit by anyone with school science. This series is directed at a more advanced level, and hence my opening remark.

R. P. DALES